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ORIGINAL LECTURES.

ON THE MUSCULAR REACTIONS KNOWN AS TENDON-JERKS AND MUSCLE-JERKS.

*A Lecture Delivered at the Infirmary for Nervous Diseases,
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IT has long been my wish to give in this hospital a series of lectures on certain symptoms of nervous diseases. This would have included what I may call their natural history—the modifications they exhibit in various diseases and in different people. Circumstances will interfere with the full development of my project, and I shall this May be able to give but one lecture. For this I have chosen muscular reactions in health and in disease, and, limiting our study to mechanical stimuli, I shall have little to say as to electricity.

There are symptoms which exist in health and are changed or abolished by disease. There are reactions known only to disease. Most of the muscular reactions are present in the healthy, and are merely altered by sickness. I have selected them for treatment in this lecture because a full enough statement of recently acquired knowledge as to them is not to be found in the text-books, and because with the material in our wards it is easy to illustrate my subject.

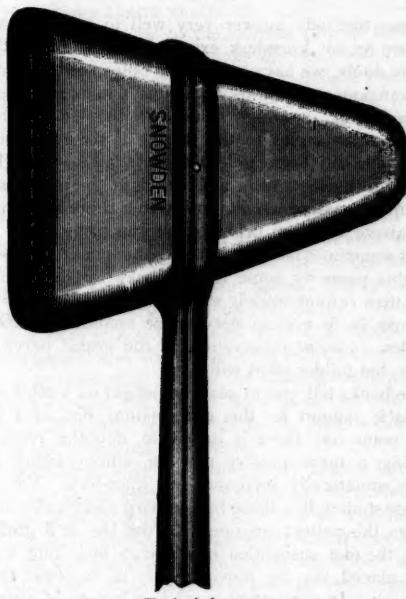
These symptoms, which are also facts of health, include what results we get from striking a tendon, and those which follow a blow on the muscular tissue.¹

If the tendon of a muscle be abruptly pulled upon, the like effect is conveyed through it to the muscle and this responds by a contraction, which in turn pulls on the tendon and moves the connected part. In certain morbid states any muscle can be made to react to a blow on its tendon, but in health, with less sensitiveness of muscular reply, only favorably situated tendons can be so used with certainty of response. The tendon must span some region of soft tissue, so that being struck while tense, it may be driven down in a loop, and thus occasion a quick jerk of the muscle. The lower tendon of the patella and that of the gastrocnemius are best situated; the biceps at the elbow, not quite so favorably; and the temporals reply to a blow on the lower jaw.²

Tendon-jerks may be had elsewhere even in health, but care is needed, and peculiar forms of hammer. Indeed, for accurate study of any tendon-jerk the hammer with a head of not too hard caoutchouc is desirable. Dr. J. M. Taylor has devised the one I show you, the long side for use on tendons, and the round end to get muscle responses by a direct blow on the muscle (Fig. 1).

It will be easiest and best to begin with the study of the knee-jerk or knee-phenomenon, as that was the first of its class to be clinically utilized, the one best known, and most fully studied.

Fig. 1.



Taylor's hammer.

Let us see, in a normal man, how readily to evoke knee-jerk, or what we briefly call K. J.; how to measure it; what modifies it; how to explain it and its variations; and, lastly, what clinical value it has. When we have covered this ground as to knee-jerk, the other like phenomena will demand little additional explanation.

To test roughly the mere presence of knee-jerk, it suffices to set the patient on a table's edge, so that the legs may swing, or to let him cross his legs so as to allow the upper limb to hang passive. Then strike, with the side of the palm, on the tendon of the knee-cap above its tibial attachment, a smart, quick blow. The tendon thus abr. ipty jerked communicates its motion to the muscle, and this responds in mass by a contraction which moves the leg and foot. A prettier way is to put the left forefinger on the tendon, and then to strike with the other hand, or, better, with the hammer, upon the finger so placed. The faintest response is felt by the finger while the eye may observe also the lift of the foot.

This plan is very useful for examining a man on his back in bed. You properly flex the knee, and with the man's foot at rest on the bed, put a finger on the tendon and strike on it, judging by the sense of touch the

¹ For the best account of them see Buzzard's admirable essays, and Ross, p. 162, vol. i.

² First discovered in this hospital by Dr. Morris J. Lewis.

amount of the knee-jerk. Very often I seize the extensor mass with the left hand and strike the tendon with the ulnar edge of the right hand. You thus feel the muscle as it swells responsive to the jerk made through its tendon. In rapid examinations, in women, it is usually easy to decide as to the mere presence of knee-jerk, by striking on the finger without lifting the skirts. The best plan of all is to feel for the tendon, place thereon firmly a small rod about three-quarters of an inch in diameter and covered with rubber tubing, and strike on this. You waste no blows and cause less pain by use of this method, and can at once get the maximum effect. With this arrangement, also, it is easier to get correctly the moment for reinforcements, of which I shall presently speak.

These methods answer very well to tell us simply whether or not knee-jerk exists; but if as to this there be any doubt, we have to resort to other and more careful arrangements. In my clinic, the patient sits at ease, and well back, in a high chair, with the leg and body both at right angles to the thigh. If the response be feeble, it is still better to let the body fall back slightly, and to lift the leg a little above a right angle, so that it rests quite passive. You thus stretch the whole muscle from above and from below before you strike.

It is sometimes hard to make a patient entirely passive, and this passivity, some of the books say, is essential; but a man cannot entirely stop or conceal the knee-jerk response, be he ever so nervous or unable to relax his muscles. *Violent* innervation of the crural nerve will stop it, but milder effort will not.

The books tell you to seat the patient on a table without back support for this examination, but, as I shall soon point out, there is reason in doubtful cases for securing a more passive position, since motion elsewhere unnaturally increases the knee-jerk. For very delicate studies, like those in Lombard's admirable essay, he puts the patient on one side, the leg in a guttered splint, the foot suspended in a stirrup by a long string. Thus placed, the leg moved easily in response to the knee-jerk. In our own researches, a variety of positions were employed. In common practice the crossed leg answers, but if there be doubt as to whether the knee-jerk is either above or below the normal standard, certain precautions must be taken to insure accuracy. The need for such care will be seen as we go on to study what I like to call the natural history of this symptom. Let us observe it. As the hammer falls on the tendon of my patient, at ease on the lounge, or leaning back in a chair, the muscle contracts and the foot and leg are lifted and then fall back. We see that the excursion of the foot is less or more. As we strike again, and again, this excursion varies.

In some normal men, and often in spastic disorders, the jerk calls the opponents into action, and as Lombard shows, when the foot swings laterally suspended, and thus free, it does not always return to its place, as if the muscle did not fully relax. The primary jerk varies, but there is always a maximum beyond which the foot does not go, no matter how hard we strike.

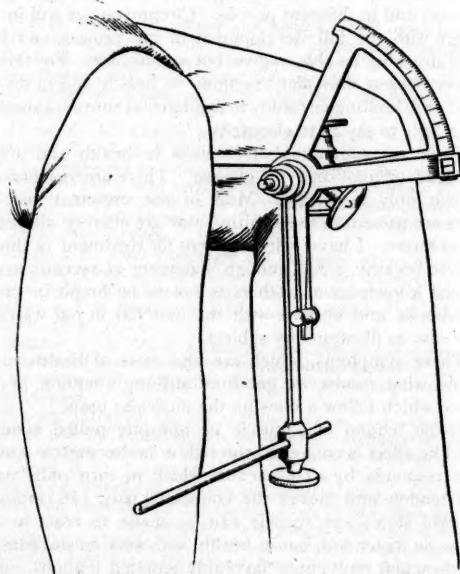
Let us further study the influence of position; I lessen the angle of the leg to the thigh, and, as you see, the repeated blows on the tendon get slighter and slighter responses. A very able observer (Gowers) thinks this is due to the fact that tension is physiologically essential to

the evolution of knee-jerk. As to this, I think him mistaken.

It is only mechanically essential (and under certain conditions it is not required at all). The tension enables us to act sharply on the largest mass of muscle. Tension is convenient for this reason. I showed long ago that a certain amount of tension, even in spastic cases, causes knee-jerk to cease. To show this, I place a spastic patient on his belly, and with the knee at a right angle, continue to strike the tendon as I further flex the limb. At a certain point the knee-jerk ceases. This is not (as you might suspect) merely because the tendon is at a disadvantage, mechanically, on account of extreme flexion, as may be easily shown, and very prettily.

The subject is seated, and the knee held at nearly that limit of flexion which stops knee-jerk; as you see, we get fair replies. Then the man is allowed to fall back supine, and the tension thus added at the truncal end of the muscle, stops all knee-jerk. You may now see why we are careful as to position, both of body and of leg.

FIG. 2.



Lombard's meter.

To get the best results out of knee-jerk, a little practice is needed. If we desire greater accuracy in our record than merely to be able to say knee-jerk present in excess or not, we have here, for clinical use, my friend Dr. Warren Lombard's simple meter. The instrument, as I now show it to you, will probably be used by neurologists only. A strong clamp attaches the instrument to a chair raised so as to keep the patient's legs off the floor. A bar which projects between the patient's legs, and which can be pulled out or shortened, carries a dial plate divided into centimetres. A light swinging rod, with a cross bar, is set so as to drop on to the middle of the tibia. The patient sits back at ease; the blow on the tendon is struck; the rising leg lifts the rod, and this carries before it an index which it leaves so as to mark the limit reached.

Two or three blows give us the maximum, and then we go on to get, also, the maximum reinforcement.

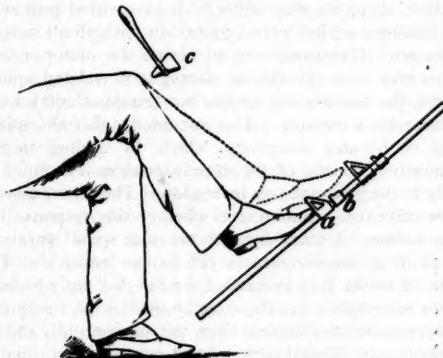
Should this instrument come into use, it would be well to agree on a common method of dividing the dial, and on a common length of lever.

Dr. Lombard's ingenious instrument, which is too costly for common use, set me to thinking how more easily to get a numerical statement of knee-jerk for reference.

With the patient always in the same position, with care to avoid reinforcements, place a yardstick or metre against the wall, in front of the foot, so that some one else can note the height to which the tip of the foot rises; get the maximum from several blows on the tendon, and note it for future use.

Here is a better measure (Fig. 3): A metre or yardstick carries a light, very movable arrangement of wire, about four inches in height, by five inches in breadth (*a*). This is set or fixed so that the toe rising with the tendon-jerk, will push it up the scale. As it moves it pushes up, and leaves behind it as a marker a double loop of wire (*b*). This I had made yesterday. It answers

FIG. 3.



The author's meter.

admirably, and in a little more complicated form will do still better. Needless to insist that the results may not be generally comparable, but, in a measure, they may be, and, for comparison, from time to time, in individual cases, are most desirable.

Our object is, of course, to get a normal expression of the knee jerk. As we avoid, therefore, too great or too slight flexion, so also do we avoid all other influential agencies. Thus, if it did not take too long, you might easily be made to see that pressure on the sciatic nerve, —constriction of the leg (even so slight as that made by a rolled-up pantaloons) serves to lessen knee-jerk. Even fatigue may do this. In my own case, and in others, the knee-jerk is least at night and best in the morning. Moreover, damp, relaxing days lessen it notably, and always after my winter work it is not so good as in the autumn after my holiday. It is different also in most men in the two legs. On the other hand, several things increase the knee-jerk, and may cause it to seem great, when, without such aids, it is not. These influencing additions were called by Dr. Morris Lewis and myself "reinforcements."

What these are—how to avoid their deceptive presence—how also to use them, will be the next subject to

which I now ask your attention. About this matter more is known than was at our disposal three years ago. Much of what is known was contributed by members of the staff of this hospital, and the four papers which have cast a flood of light on the phenomena of knee-jerk are, I am glad to say, entirely the work of American physiologists.

The germination of thought in crops is often interesting. For years there had been no large addition to our knowledge of knee-jerk, when Jendrássik of Prague made the most interesting observation that certain *violent* motor acts increased the knee-jerk, if they took place in time nearly coincident with it. That he should have here left this fruitful fact without further study seems amazing. Still more that it should have remained for some years almost unnoticed.

Turn with me now to the normal subject before us, and let us illustrate the strange harvest, which for Dr. Lewis and for me, grew out of Jendrássik's pregnant observation. I shall show that every motion, if at all decided (even if it involve a small single muscle), increases knee-jerk. To wink, speak, cough, move a finger is enough, as you see. Probably all coincident motions thus reinforce knee-jerk in degrees which vary with their intensity. A vast range of facts in these directions was evolved by us out of Jendrássik's single statement. Just now they interest us on but one side. Presently we may discuss their cause. Enough at present to show that they teach us two things. First—that when a knee-jerk is seemingly absent an added motor reinforcement may show it to be not entirely lost. Second—that in estimating normal, or other knee-jerks, if we desire accuracy, the patient must be at rest with closed eyes, and neither speak, nor laugh, nor swallow, nor make any avoidable movement.

But while we were thus enormously adding to the singular fact made known by Jendrássik, we discovered a new set of reinforcing agencies. Just as it seemed that all voluntary motion apparently increased knee-jerk, so also, we found that a vast range of sensations had the same power. This discovery has, as yet, been of no direct clinical value. It has, however, been shown by us of late that in certain cases, motor reinforcements fail to act, and sensory reinforcements remain efficient. In others the reverse obtains, and it is to be hoped that as we get clear ideas as to the spinal tracks of reinforcements such knowledge as this may become valuable.

To exhibit an illustration of sensory reinforcements, I strike the tendon lightly, so as to cause a gentle foot movement. Just before the next blow I touch the skin anywhere, on the arm, neck or leg, with ice or a hot blade, or pull a hair or pinch the skin. The results as seen are startling. There seems to be a minimum limit of sensory reinforcement. A touch does not answer, or seems not to do so, and yet, probably all sensory impressions, like all motions, act in this direction. Not all, however, have power visibly to represent their effects. The most tremendous influence is to be had by passing galvanism through the anterior part of the brain, and if we add to this spontaneously also a sensory and a motor reinforcement we get such knee-jerks as elsewhere are seen only in spastic disease. Galvanism, thus used, has probably a complex effect, made up of sensation, motion, emotion, and some other more immediate affections of the brain.

In practice I find you can get the united influence of sensory and motor reinforcements by asking a man to pinch his arm at a signal; if, then, also he should make a grimace, you will have powerful reinforcements of two kinds.

When you come to suspect that a diseased zone of cord may probably reinforce parts below it, such sensory increments become most interesting to think of.

But let us turn, for the time, to the next crop evolved by the work of Lewis and myself. My friend Dr. Warren J. Lombard took up the subject, and with accurate laboratory methods, confirmed all of our results, and added many things of interest. He showed, as we had done, how fatigue of body lessens knee-jerk, how sleep affects it, and how the weather influences it. We come to see, as we read these two papers, that this responsive act represents well the state of the body. For reasons to be later stated, it is as yet clinically valueless in this direction, because of its too delicate cognizance of actions, sensations, and emotions. Indeed, the most brilliant discovery of Lombard was that "emotion enormously affected the knee-jerk." If you listen to music, or read poetry and a passage touches you, the knee-jerk obtained just then is vastly reinforced. A cry of a child, a sudden knock at the door suffices to do the like. And all this is to be remembered, because it gives some clew to the mechanism of reinforcements, and because of the influences of emotion in cases examined, especially for the first time. In fact, emotion seems to be, of all reinforcements, the most remarkable, save galvanism of the head. When, for example, you have before you an hysterical girl, what with fear, excitement, apprehension, and perhaps shame, there is an emotional state which seems strangely competent to exaggerate the knee-jerk, and to do so for some considerable time, making what I might call a quasi-permanent state of reinforcement.

In Lombard's experiments it was brief, transient emotions which were thus active, and he was not called upon to see, as we are, the whole spinal system thus excited for half an hour or more by the grouped excitatory power of many emotions. Every neurologist sees this condition. You strike the knee in an hysterical girl and the knee-jerk is surprising, and with it often enough the other leg flies up, and one arm—usually the left. A blow on the tibia will not do this as a blow on the tendon does. It is a combined result of a blow on the tendon and of emotion.

We see the same thing in the first examination of neurasthenic, timid and apprehensive men. At another visit, the exaggeration is missing. It has gone with the subsidence of emotion, or has much lessened.

I have enumerated most of the known agencies which lessen or increase knee-jerk. There may be many others, and Lombard's study and our own show that in all likelihood the interior operations of the body, and even of the mind, may furnish reinforcing agencies which we have no means of analyzing or explaining.

I have dealt thus far only with the natural history of the evolved symptoms of knee-jerk. What, now, is the explanation of this occurrence?

All muscles have a certain capacity to respond by motion to the two means of excitation known to us. These are, first, mechanical, as a blow, or a sudden pull on the muscles through the tendon; and, second, the various forms of electricity.

Let us deal with the first. I strike a blow on the tendon of the patella below the knee. The crural mass of muscle answers by motion. Again; I strike just above the patella—we are still on tendon—and the muscle moves in mass. I go higher, and the extent of the movement lessens as I leave the tendon. Now, I turn my hammer, and strike with the point on the muscle. There is still motion, but it is limited in amount, and as to the area affected. On the thin chest muscles, with bone beneath them, you see best what occurs. The blow falls; a certain length of fibre shortens, and in some cases, and always in certain situations, just under the hammer a hump of muscle rises slowly, and as slowly falls away. If all at once you could strike every fibre of the muscle, you would have a result in effect like that which the favoring relation of muscle to tendon enables you to get when you strike the latter. So far the two sets of motor responses are much alike; also, there is a further resemblance. Both are reinforceable by distant motion and by sensation. Strike lightly with the rounded hammer point on the supinator longus, and at the next blow tell the patient to shut the other hand strongly, and you get a visible reinforcement.

How, then, do they differ? Both involve primarily the presence of that intrinsic irritability which all muscle possesses. If disease or injury severs the motor or sensory nerve of a muscle, or damages its related spinal centre, the tendon-jerk ceases, but the muscle-jerk from a direct blow remains. The inference is that the spinal cord contributes something, which, by adding to the responsive capacity of the muscle, enables it to move in reply to the pull made on its tendon. The direct muscle blow still causes motion, and whether this response be, in a measure, *lessened* by such nerve or spinal paralys as *destroys* tendon-jerk, is yet to be learned.¹ The same elements may be needed for the *full* reply to both forms of stimulus, but the direct blow is what I may call a more positive stimulus than the tendon-pull, and so evokes a reply from the intrinsic unassisted muscle quality of excitability. This property of muscle may even be increased by disease which destroys the knee-jerk. The nerve or spinal loss which acts thus destroys for both tendon-jerk and muscle-jerk the capacity to be reinforced by distant motion or sensation, so that here, too, is an added resemblance between muscle-jerk and knee-jerk.

And now as to the mechanism, which, in health, causes a muscle stretched abruptly through its tendon to contract. The mere intrinsic muscular irritability will not suffice. That gives the muscle power to contract under a blow, or from electricity, but something more is wanted to give rise to the response to a pull. What is the addition which supplies this excess of excitability? Whatever it be is present in excess during reinforcements, and whatever explains the mere knee-jerk must also, in a measure, explain reinforcements.

We are thus brought to the much disputed question as to whether knee-jerk be a direct muscle act or a reflex. At first, it seemed simple to suppose it a reflex from the

¹ Thus, if we conceive that both a direct blow and tendon-pull act alike, and both profit by the spinal aid, this latter being taken away destroys the tendon-jerk, but leaves us able to see a muscle-jerk, because, even if this loses something by loss of spinal connection, the mere intrinsic muscle properties enable violent local stimulation to get responses. To prove that something is thus lost, for it is but theory, we have yet to study the facts with more care.

tendon, but when all tendon nerves are cut it still occurs. Then it was presumed to be a reflex, due to the sensory effect of the multiple pulls on the nerves of the muscular aponeurosis, through the tendon. This was supposed to cause excitation of the spinal cord, an efferent motor response ending in a muscle act. This would be an ordinary reflex act. But, as against this, was the fact that the time of a tendon-jerk is not less than a fourth that of a common skin reflex. This objection seemed final to most physiologists as against the reflex theory. And yet, some spinal aid was clearly requisite. An able English neurologist (Gowers), deceived, as I think, by the mechanical needs of the knee-jerk, came to believe that tension was the precedent requisite, and that preparatory passive tension, obtained by flexion, excites by a reflex influence a state of extreme irritability to local stimulation which enables knee-jerk to occur. In other words, there is a precurory tuning of the muscle.

My previous remarks show that tension is less needed than Gowers supposed; and of late, his ingenious explanation has received less acceptance.

Another view presumes that the muscle is kept constantly in a state of sensitiveness by contributions of excitation from the spine. Out of the influence of these, *plus* the normal excitability, arises the so-called tone of the muscle. It would be lost by nerve injury, sensory or motor, and by certain central diseases, and with its loss the increment of sensitiveness due to spinal contribution would fail, and with this of course, too, the knee-jerk.

All reinforcements would be explained as coincidentally supplying power to these ever-flowing tone waves. Under this explanation, knee-jerk becomes a local muscle-jerk, and its brief time not so surprising or embarrassing. When I last wrote of this matter, we were inclined to accept the tone theory as explanatory of all the facts; but, in the light of Lombard's and Bowditch's work, and my own further reflections, I have come to doubt more and more, and to feel it as possible that knee-jerk is really a peculiar reflex, with a short time. To settle the matter we require fresh laboratory studies of muscle-muscle reflexes in man.¹

As regards the tone theory, it seems hard to conceive that instantly on nerve section the muscle should be so toneless as to give no knee-jerk, and there are other opposing facts. Section of the motor nerves through which the tone waves flow (if they exist) paralyzes knee-jerk; but section of sensory nerves does the same, and it is hard to explain this, unless we conceive of the individual muscle metabolisms as furnishing to the spine the essential excitations which return from it to the muscle in the tone waves.

If tone, in the sense here explained, be not a competent theory, then we must consent to call knee-jerk a reflex, and consider all reinforcements as not going beyond the spinal ganglia, and as therein reinforcing reflex excitations, in place of spreading out as tone waves to the muscle, and therein reinforcing.

Tone has been taken to mean tension, or some more mysterious preparation, for response to excito-motor agencies. If it be tension, this ought to be capable of being shown. I spent a great deal of time in efforts to see, if

with the leg at an angle and without a blow on the tendon, reinforcements would not show themselves. In this I failed entirely. I do not say it is not to be shown. But if tone waves reach the muscles, and are the positive things they seem to be, they ought to be capable of registration. Certainly they cannot mean tension; and, if not measurable in muscle, it would look as though they did not reach it.

Time may settle this question. Meanwhile, as against reflex theories is the time. As to this, however, it is unwise to be sure that skin-muscle reflex time is the only one. Muscle-muscle reflex time has yet to be more fully studied, and may be more brief than has been supposed. We were quite unable to reinforce skin-muscle reflexes, such as that of the cremaster. The study was excessively difficult, and I do not yet feel absolutely assured that we were correct. If it should prove that we were, this might be regarded as measurably against the view that the muscle-muscle acts (K.-J.) are true reflexes. At all events there seems to be this difference between the two sets of replies to excitations.

There are other points which look favorable for the reflex theory, or as against tone explanations. Thus, when we cut the motor roots which enable knee-jerk to occur, we cannot restore knee-jerk thus lost by substituting a delicate electric current in the peripheral nerve end to imitate the lost spinal contribution. Again, a blow on the tendon causes not only local, but sometimes also distant reflex muscle acts in the other leg or in an arm. These cannot be skin reflexes,¹ and are seen best in the presence of emotional reinforcements.

Again, it often happens to one to strike a tendon with equal force over and over, and only after a time to get a violent response, far beyond the man's normal. I have seen this in tired men. It appears to be a summation of excitations and an explosive reply. I find it hard to conceive of this as a localized muscle phenomenon. It is like a reflex occurrence. A blow on a muscle elongates a limited breadth of fibre. It contracts, and this is like a knee-tendon jerk, but is a limited and coarse reply. If knee-jerk depended only on perfect extra-spinal muscular excitability, when this quality rises, as it does in some diseases, the knee-jerk ought also to increase, but in these very states it may be lost, and yet, the answer to a blow remain above the normal.

We were never able to reinforce muscle acts electrically caused. As Professor John Curtis suggested to me, this may be because electricity is a less potent reflex excito-motor than mechanical force. If the reinforcements reach muscle as tone waves, one excitor should be as good as another; but if they do not, then the only agents which will reinforce will be such as competently reach along reflex nerves to the spine, and it would look as if a blow did this, and moderate faradic currents did not.

There is, indeed, already some reason to suppose that mild electric currents are less competent reflex excito-motors than mechanical force. Thus, while a mere touch on the side of the thigh will excite the cremaster reflex, a faradic current strong enough to evoke muscular motion may not cause the testicle to stir.² The argu-

¹ A blow elsewhere does not cause them.

² To test this place both conductors dry on the excito-cremasteric region. The first cool touch may cause cremaster reflex. Then, being careful not to move the conductors, make and break

¹ I should then propose to use these terms, muscle-muscle reflex and skin-muscle reflex.

ment as concerns electricity and mechanical stimulus is emphasized in spastic cases. In these, the faradic electric reactions are usually not increased, while a tap or squeeze or tendon-jerk is competent to excite extensive movement. The blow gives rise to excessive action, the electricity only to common responses.

I have stated the gathering arguments for and against, but after all has been said, we are still in doubt as to the true nature of tendon-jerks. Yet, as the matter now stands, it is more and more clear that the time element is not the convincing argument against considering the knee-jerk a reflex, which many writers have held it to be.

Since Jendrássik's discovery and its extension by Lewis and myself, with our additional discovery of the reinforcement power of sensations, and Lombard's emotion reinforcements, the question of the nature of these has also to be considered.

There seem to be but two possible ways in which motion, sensation, or emotion can reinforce. One is indirect and one direct. It may be that all motor or sensory activity tends to release from inhibitory control other ganglionic masses than those in immediate use, and sets them in a measure free to respond to other agencies, such as external stimulus. To use a homely comparison, "the brakes are taken off." Thus a motor volitional act may so affect a spinal centre as either to set it at liberty to send out more energetic tone-waves, or more potently to feel, or to feel and reply to reflex excitants. The other view is that every motor act or sensory impression sets free surplus energy, which is felt as reinforcement throughout the body. And this is the doctrine of overflow.

I have already pointed out that every positive and abrupt motion or sensation, however remote and wherever it may be, is able to reinforce any tendon- or muscle-jerk. What is true of the stronger influences must be measurably true of the weaker. The reinforcing excitations may not show in the increase of the jerk, and yet may exist. Accept this quite possible conception that every sensation, every volition, every emotion (perhaps every mentation) sends its excess of force over countless tracks into many parts of the nervous system, and possibly into every muscle. The effort to realize this incessant effluence of energy is difficult. Some attempt at analysis may be of use, and I would also like to point out what are the problems, for the solution of which we must look to the laboratory and clinic. When we will a motion with some positiveness, the knee-jerk is reinforced, and so is the muscle-jerk. This happens whether the muscle called to act exists, or has been lost by amputation, or is above the knee in walking, or below it. If it were only to occur when the muscle is (let us say) in the feet, or below the knee, we might think of the nerve influence as passing through the crural centre, but a wink does it even better. How then does the efferent force reach the knee-centre? Is it a mere overflow, or is it an inhibitory act releasing the crural centre from cerebral control, and what columns of the spine must in any case rest uninjured to enable it to occur? Also what is the time of this reinforcement?

Then, as to sensations. I pull at a hair on the arm, and get prompt reinforcement of the knee-jerk. In this

circuit (faradic). A current powerful enough to excite the muscles will be found up to a certain point comparatively incompetent to occasion reflex stimulation of the cremaster.

case does the afferent impression reach the sensorium first, and then affect the knee-jerk? Is the time of motor and sensory additions alike, or how nearly? Does the pinch of a foot influence the knee-jerk as the afferent impression goes up the spine, or does it first reach the sensorium, and act thence downward on the centres finally concerned in the knee-jerk?¹

Emotions increase the knee-jerk. Do these act so as to set the spinal centres free, or is their influence also one of overflow?

Thus at every step the question of overflow or inhibition faces us, and in disease it is still the same. Certainly in health either explanation seems competent. But in disease and injury this is hardly so. When we cut the cord a state of shock, so called, occurs, that is—the reflexes and the knee-jerk are lost for a varying time. This may be merely exhaustion from a tremendous hyper-physiological excitation. Recovering, the knee-jerk and reflexes get more and more sensitive, and, at last, reach the normal and rise far above it. Why do they go on increasing, unless the pathological changes at the point divided act as irritative reinforcing foci? Again, in some spastic palsies, the knee-jerk being enormous, it is easily reinforced by winks or hand acts. It is hard here to conceive of the cerebral influence as largely cut off. Indeed it is not. Volition exists and is really good, but not competent because of the rigidity it causes. Generally, in all irritative spinal maladies the knee-jerk rises before destruction of the centre checks it. Also, after section of the cord, inhibition does not explain the gradual increase of excitability, while it is explained by the coming on and increase of inflammation at the lower surface of the section and the sequent changes.

Suppose, now, we can show that one volition reinforces another—i. e., that the tired right hand gains vigor from sudden coinstantaneous use of the left hand. Either this is overflow, or else the willing of motion in the untired left hand must be supposed to relieve the tired centres from inhibition, and thus enable them better to obey the will they are failing to obey. If we accept overflow to explain such volitional reinforcement, there is no reason why we should not do so in regard to knee-jerk reinforcement.

Men who hold theoretic explanations too tightly, are apt nowadays to get sharp lessons. Yet, on the whole, it is often wise to possess some binding explanation of groups of facts. It is helpful in many ways. Possessing no certainty, I most incline, for the present, to look upon all reinforcements as overflows, and to consider all abnormal increase of knee-jerk as the product of irritative lesions. Physiologically, the conception of innumerable effluences of energy, motor, sensory, emotional in endless activity fills us with wonder and tempts one, however we explain them, to speculations remote from the purpose of this lecture.

Before dealing with the little there is to add as to ten-

¹ In the case of fractured spine (spoken of later in this lecture), there was every reason to believe the paths from leg to brain, and from brain to leg, were cut off. I examined this case with Prof. Osler, and both were satisfied of the completeness of the isolation. At last, after many blows on the right knee tendon, it began to respond better, and then we found it could be reinforced by a grimace, so that here was a path open from brain to leg. Also, it was not reinforced by a sensation (cold from ice). This seems to be a novel use of reinforcement.

don-jerks elsewhere, I desire to allude to the last facts which have been born of Jendrássik's discovery. My friend Prof. Bowditch, of Harvard, has been studying with care the time of reinforcements. His result is most interesting. I quote, by permission, Dr. Bowditch's brief summary of his paper, which is not yet in print.

"A sudden, brief muscular contraction reinforces the knee-jerk when the latter occurs within a brief interval of time (less than 0.25 second) after the former. With a longer interval the reinforcement gives place to an inhibition. With a still greater length of interval this inhibition disappears, so that when the blow on the tendon follows the muscular contraction with an interval of 1.06 second, the knee-jerk has its normal value. The interesting feature of the result is, to my mind, the evidence of a sort of oscillatory activity, or rather change of irritability in the centres concerned in the knee-jerk. I attach no particular importance to the figures I have given. They will doubtless have to be changed when more observations are accumulated. It would be interesting if this period of oscillation of the nerve centres should be found to differ in different persons, after the manner of the personal equation of astronomers."

This discovery that a sudden brief reinforcement first increases knee-jerk and then lessens it for a brief season is curious. The latter may be read as a brief expression of fatigue in the centre, or as interference of nerve waves; coincidence with increase, interference with decrease of knee-jerk.

Let us now turn again to the muscle-jerk. When a blow falls on a muscle, is the response purely the answer of intrinsic muscular irritability? This is a question which so far seems not to have been asked. The muscle shortens when struck, even though without spinal connections; but as I have already said, it may be that so long as these are whole, muscle-jerk may be like knee-jerk, and profit by spinal contributions to its coarser phenomenon. If it be a double phenomenon, its curve should show this fact and exhibit a double ascent, since the time of the muscle-jerk and its spinal addition cannot be one.¹ If the whole matter is but a response from a toned muscle, then we should have but a single curve.

The direct muscle-jerk must be familiar to all the observant among you, as seen easily when the percussion hammer strikes on the thin chest walls and instantly the pectoralis fibres, for about the width struck, contract from end to end. They have been violently pulled upon by the indenting hammer; a coarse, powerful excitation. You observe their responsive contraction. Next, at the point struck, you see a small hump quite slowly form and as slowly disappear. The first phenomenon you can see in all muscles more or less well; the second is not so easily obtainable. As concerns the first, it varies as do tendon-jerks. It is reinforcing and obeys the same laws as these do. In the muscle backed by bone, as on the chest or over the scapula, it is well exhibited; but also in other muscles it is well shown. The arm muscles are especially sensitive, and it is possible to pick out muscles as we do with electricity.

In the leg it is more difficult to get as good responses, except in thin people; and here, as elsewhere, we get exaggerated effects, near to the "nerve-points" of Ziems-

sen. To get the best results slight extension is needed. The pull on a length of muscle can only be had when the tissues are so mechanically stretched as to enable a blow to pull sharply on a length of fibres. Extreme tension lessens the response. In the feeble, and in the tubercular, the direct muscle-jerk is best. Partly, this is due to the great effect of blows on uncushioned muscles. Partly it is, I suspect, physiological, but whether from increase of intrinsic excitability, or from spinal contributions is not known. In some diseases, as in ataxia, it increases.¹

The hump which slowly rises at a right angle to the longer rise of a length of fibres is the direct effect of the impact. Over muscles backed by bone we get it best, in either health or disease. It is in its larger expression and where found in other muscles than those mentioned, more or less, a sign or an accompaniment of feebleness. If we kill an animal, this response to a blow increases for a time, and in some diseases, with nervously isolated parts, is increased. Stokes mentioned it about 1848, and I experimented and wrote upon it in 1858; and often since it comes up as a re-discovery.

In section of the spine, or fracture with like result, the muscle changes are curious. I show you an illustration from one of Dr. Osler's cases: here is a fracture of the spine, at the ninth dorsal vertebra. It took place early in April, 1888. There is as you see, no sensation for pain. Pin pricks do not bleed, for this is not, as described, to be found only in hysterical anaesthesia. There is no voluntary motion.

I strike the leg-muscle anywhere, it moves better than usual in its length, but, also, the local hump is huge. I never before saw it so remarkable. Even a pinch causes it. The observation is to me a novel one. And observe that there is a spot of local gangrene on the heel. Some undamaged ganglia exist below the seat of injury, or we would not have even the faint knee-jerk seen after several blows on the right tendon. What is the reason for the peculiar irritability to a blow? Electrical tests show, I am told, no remarkable degenerative reaction. Possibly there may be an increase of muscular irritability from chemical irritative changes in the muscle, but the whole subject awaits more careful study.

In practice we have to deal only with ankle-jerk, knee-jerk, and biceps-jerk, and chiefly with the latter as a means of learning when certain spinal maladies are invading the arms. As concerns biceps-jerk, I hear men complain that they cannot get it. Let the patient rest the bared limb on the arm of the chair in which he sits. The operator feels for the insertion of the biceps tendon; puts on it a finger and strikes upon that, thus. He at once feels in the finger the muscular response. In thin persons a blow directly upon the tendon causes the arm to jerk visibly. Sometimes I put on the tendon a bit of caoutchouc, and strike on this firm cushion.

Pay attend to the fact that the biceps tendon, like the knee-cap tendon, must be stretched to enable us to influence the attached muscle. Elbow-jerk is certainly less easy to get than knee-jerk, but it is worth looking for, and is often a valuable aid to prognosis.

While much care is apt to be given to the presence of clonus, a symptom only as a rule present in excess in

¹ This is, of course, if the knee-jerk and measurably the muscle-jerk be reflexes.

Buzzard first pointed this out in an admirable article on knee-jerk, from which some of my arguments are taken.

disease, the normal expression of it, ankle-jerk is, I fancy, less sedulously studied. The usual mode of evolving ankle-jerk is to strike a smart blow on the stretched tendo Achillis. The patient being seated, or supine, take up the foot in the left hand and flex it so as to make slightly tense the tendon on which you strike, and preferably with the hammer. If there be no response, flex the leg slightly and repeat the blow. The reaction of the muscle is felt by the hand which holds the foot. To show it better, I loop a thin rubber band over the tip of the sole, and with it make a slight extension. As now I strike, you see the foot move because of the contraction of the abruptly stretched gastrocnemius. I take another case, and show you another method which I do not see elsewhere used. It seems to be new. With the foot as before, I strike on the sole of the foot. This, also, acts so as to make a quick pull through the tendon on the muscle. It is less awkward than the blow on the tendon, and shows best with the boot or shoe on the foot. In the books you will see mentioned what Gower calls the "front tap," which now I show you. The foot being flexed, we strike a blow on the anterior muscle, or with less effect on the tibia. You see that the foot moves, showing that the calf muscles have acted. Gower mentions this as seen when the phenomena are in excess, but, as a fact, this reaction is common enough in the healthy, if the blow be given near the ankle. I believe that the front tap phenomenon has been sometimes considered as positive evidence of organic disease of the cord. As I now show it to you, it is strongly marked in spastic disorders, and more mildly expressed in some well people, or in neurasthenics.

And now as to clonus. It is easy to illustrate, but, in some cases, needs a little care to develop. To get it, I place the subject (a well man) so as to have the knee a little bent, then I suddenly flex the foot. You see I get no answering motion. This is because the flexion so made is not abrupt enough to secure even a single reply, for, remember, a sharp blow on the tendon or sole does usually secure this in health. In a spastic case, like this boy, who has Pott's disease, the apparatus involved is excitable enough to go on responding so long as I keep up flexion of the foot.

See what happens. I push up the foot and the stretched muscle moves the foot. My continuous pressure once more stretches the muscle. Again it moves, and so on, till the power to reply is exhausted. We may substitute a weight, or an elastic attachment, for the hand and see the motion continue for an hour or more. Here is a set of curves written by causing the foot to record its motion on a moving sheet of smoked paper. A second marker notes the time. You see the clonus is seven to the second. It varies from five to nine, and the time of the two feet taken together varies a little. It causes no fatigue, although moving for an hour at the rate of 420 to the minute.

In milder cases it ceases after a few movements, but we never see clonus continuous and energetic save in true spastic cases. In milder examples, as in hysteria and neurasthenia, we must consider it as to amount and vigor, and as to its presence with other symptoms. I now speak of what you get by using the hand alone, and keeping up pressure. You may sometimes, though rarely, get it in a mild form in health—that is, in place of a single response to the blow on foot or tendon,

two to six, such motions occur, each slighter than the last. There is, of course, in all this a vast amount of physiological interest upon which I have dealt but slightly.

I come now to say what amount of practical use we can get out of the varied phenomena I have described. Keep clearly in mind that if you find a tendon-jerk lessened or extinct you must try if it can be revived or increased by motor or sensory reinforcements. Absence is most suspicious, but do not let this alone make you decide that you are in the presence of coarse spinal lesions. I know two ideally healthy people, who have no knee-jerk, ankle-, or biceps-jerks; no reinforcements. Both have direct muscle-jerk, non-reinforceable. Explain this, I cannot. I cannot even guess at an approach to an explanation, but it is enough to make one careful. Taken alone, this negative sign has little value. It usually means, when present with other suspicious signs, that the nerve loop from muscle to spine, and back to muscle—*i.e.*, the muscle-muscle loop—is damaged in the spinal centre, in the nerve loop afferent or efferent. It may mean, also, that the muscle has itself lost power to respond. To know what has suffered, we must appeal to other symptoms. It tells us that something is wrong, but not what is wrong. Thus it lessens, and at last is lost in posterior sclerosis, and in sclerosis of the gray anterior columns, and also in hypertrophic muscular palsy, thus affording illustrations of loss of knee-jerk from disease of either the sensory or motor columns, or the muscular mass. I do not see that, as yet, we have got out of the knee-jerk changes all that we may yet hope to win. Except in locomotor ataxis, and in the early recognition of spastic states of the cord, it has little direct practical usefulness. But it has a negative value, and a great one, owing to its remarkable sensitiveness. Let us deal first with its negative utility.

It is a common case to see, nowadays, a man of thirty, free from constitutional taint, or with a doubtful history, ruddy, and well nourished. Otherwise he is easily tired, and this may apply to body and mind, spine and brain, or to either. He has fugitive numbness of the extremities and ache in the cervical or lumbar spine. Beyond this he has no spinal symptoms, and his knee-jerk, ankle-jerk, and elbow-jerk are perfect. You may be sure that he has merely neurasthenia. But if he be very emotional, and this exaggerates his tendon-jerks, and even causes slight clonus, you may have to wait long, and be careful in order to get undisturbed results. It is an every-day experience that in suspicious cases integrity of tendon-jerks helps us to clear views. It is quite certain that some cases of posterior sclerosis begin with excessive knee-jerks, which by and by fail, and at last disappear. For a time they can be recalled by reinforcements, but, sooner or later, are lost altogether.

An interesting case of ataxia of the arms with nearly normal legs was recently in our wards. I use it here to illustrate the history of tendon muscle-jerks in ataxia, and also to call attention to the way in which we state with brevity such phenomena.

Modern note-taking is so elaborate that whatever rationally shortens its method is a pure gain. Let me point out how you can record these phenomena briefly in a sort of equation, and refer you to the paper by Dr. Lewis and myself for fuller statements.

I give below a sufficiently full explanation of the abbreviations:

Arms : R. E. J. o. Rt. o. L. E. J. o. Rt. o.
R. M. J. + +. Rt. o. L. M. J. +. Rt. o.

Legs : R. K. J. 25°. Rt. 40°. L. K. J. 27°. Rt. 36°.
R. M. J. quadriceps, n. Rt. n. L. M. J. n. Rt. n.

Station : $\frac{2}{A}$, $\frac{1\frac{1}{2}}{D}$. Eyes shut.

Explanation.—Arms: Right elbow-jerk, none. Reinforcement, none, etc. Right muscle-jerk, excessive. No reinforcement, etc. Legs: Right knee-jerk, 25 degrees Lombard's meter. Reinforcement sends lever to 40 degrees, and these records mean maximum of several trials. As concerns muscle-jerk, n means normal, + excessive, and — less than normal. Station $\frac{2}{A}$, $\frac{1\frac{1}{2}}{D}$

means the anterior sway is 2 inches, and the dextral $1\frac{1}{2}$.

Of course, in the not rare examples of posterior sclerosis with lateral column involvement and spastic gait, there is exaggerated knee-jerk, but also, I am sure, as I have said, that excess of knee-jerk may exist in the pre-ataxic stage of unadulterated disease of the posterior columns.

As Lewis and I have urged, it is in the arms of men who have ataxic legs and no knee-jerk that we can follow the whole series from the normal to lost elbow-jerk. And so also muscle-jerk +, with + reinforcements through to loss of all but intrinsic muscle-jerk. I saw, to-day, an ataxic man. He has knee-jerk o. Reinforcements o. Arms, elbow-jerk and reinforcements + +, and muscle-jerk and reinforcements + +. He has no ataxia of arms, but the faintest traces of occasional numbness in the finger-tips. With shut eyes he falls.

I believe that attention has not been called to the fact that, now and then, in diseases a blow on the patellar tendon causes response in but a part of the biceps mass. I saw this lately in a case of partial destruction of a segment of dorsal cord. For several days, only the *vastus internum* seemed to act, and that feebly. My colleague, Dr. Sinkler, has made a similar observation recently in a case of syphilis of the dorsal cord. Only the external *vastus* responded.

I cannot enough warn you against regarding even entire loss of knee-jerk as always a grave matter. At the same time, it is a sign not to be neglected. The sensitiveness of knee jerk, its delicacy, makes it valuable. But illustrations are better than mere general statements. I saw last week a case of mild non-febrile muscular rheumatism. The patient was in bed and under daily use of sixty grains of salicylate of soda. For a week or more he had much lessened right knee-jerk, and none on the left, and no reinforcement. It came back a few days later. The cause of the loss I do not know.

Again, a case of frank meningitis in a lad of sixteen was rapidly relieved. The knee-jerks were good—rather excessive. About the fourteenth day a dull, but very painful ache in the lower dorsal region came on, chiefly on the left side. There was, also, at this time, a sudden albuminuria without signs of congestion. I was not secure as to the cause of the ache, until I observed entire loss of left knee-jerk. I then felt pretty certain that the pain was due to a new focus of meningitis. There was little else to help me, for all through there

had been slight pricking sensations in the feet, and this did not largely increase with the coming of the dorsal ache. I next observed, with interest, that while the left knee-jerk was gone there was excess of left foot-jerk from a blow on the sole, and even slight clonus.

There are many disorders, some maladies, and certain normal conditions in which we meet with increase of knee-jerk and ankle-jerk, or even presence of faint clonus. Of some of these I have already spoken. Only the hasty, or the unwary, could be taken in by the excess of knee-jerk, or the slight clonus of neurasthenia or hysteria. Care will, however, be needed, because it is in these very cases that emotional reinforcements are most violent, and because, also, we now and then meet with incipient spastic palsies, in which there are associated hysterical or neurasthenic states.

There are, of course, many forms of disease which give rise to spastic paraplegia, and in some of them, the earliest distinct warning is to be found in the increased knee- and ankle-jerk. This is especially the case in some of the insidious forms of syphilis, which seem, by preference, to affect the lateral columns of the spinal cord. In the absence of other symptoms which help to reveal improvement, diminishing knee-jerk in spastic disease becomes a valuable index of the change for the better in the disease which at first caused its increase. In the future. Dr. Lombard's meter or my simpler one will be of great value as giving us reasonably precise records of knee-jerk.

I have limited myself, in this lecture, to tendon-jerks and muscle-jerks in chronic neural maladies. A not uninteresting, and a little explored subject, is the study of these in acute diseases. I have said least of ankle-clonus, because, this winter, clonus in general has been the subject of careful experimental study by Dr. Morris Lewis and myself. Our paper will shortly appear.

ORIGINAL ARTICLES.

A CASE OF SÄNGER-CÆSAREAN SECTION.¹

BY JUSTUS OHAGE, M.D.,
OF ST. PAUL, MINNESOTA.

On May 29th I performed Cæsarean section, after Sänger's method, upon a rachitic dwarf, who died June 3d, from septic endometritis resp. diphtheritic endometritis.

Although the result in regard to the life of the woman was most unfortunate, the result in regard to the operation was as satisfactory as could be wished for, and fully demonstrated the value of Sänger's method, a method which has completely revolutionized operative midwifery in Europe.

It would at first seem absurd to call an operation satisfactory when the patient dies, but by a little attention to a few facts which I shall relate, it will be seen that the fatal termination was in no way due to the operation *per se*, but to a process which I fear

¹ Read at the annual meeting of the Minnesota State Medical Society, at Duluth, Minn., June 17, 1887.

has repeatedly been witnessed even in women who had a perfectly natural and easy delivery.

In our State, with its vast numbers of foreign emigrants, who in their native countries, to a great extent, belonged to the poorer laboring classes, among whom rickets is no uncommon disease of childhood, the indications for artificial delivery, on account of deformed pelvis, much oftener present themselves than in communities where the well-shaped American woman predominates. The influence of the teachings of our British brethren had caused us to try first every possible obstetrical expedient, as craniotomy, embryotomy, etc., and to look on Cæsarean section only as a forlorn hope. The natural consequence was that most of our cases of Cæsarean section were forlorn hopes before they were operated upon, and that the statistical records put us at the bottom rank with our British frères.

To Dr. Robert P. Harris, of Philadelphia, is due the credit of endeavoring to introduce Sänger's method into this country. Comparing our results with those of the Dresden and Leipzig maternities, he comes to the conclusion that it is quite time that we should follow the example of the German rather than of the British operator.

The records of the German maternities show that Cæsarean section, after Sänger's method, is not as fatal even as craniotomy, and when performed in good season, and with careful uterine closure, a favorable result to both mother and child may justly be expected. In the closure of the uterine wound lies the success of the Sänger operation, and even after his original mode of muscular resection and peritoneal undermining has been abandoned as unnecessary, it is still proper to call the operation by his name, for it was he who first pointed out the necessity of deep muscular and superficial sero-serous (Jobert-Lembert) sutures, by which alone the peritoneal surfaces could be brought into such close contact, the tension of each individual stitch moderated, and the gaping of the uterine wound prevented, through which uterine discharge could escape into the peritoneal cavity. The value of this method is shown by the fact that Germany in seven years, out of 33 cases, saved 29 women and 30 children,¹ a mortality of less than 13 per cent., while Lawson Tait, but a short time ago, claimed a mortality by Cæsarean section of over 99 per cent.² In considering this low mortality of the German maternities, we must and will hope for the time when the Sänger operation will supersede the piecemeal extraction of viable children, that it may no more be necessary to destroy one life to save (perhaps) another.

"Parce matri et proli, si parcere possis."—*Berruti.*

¹ MED. NEWS, March 26, 1887.

² MED. NEWS, July 17, 1886.

³ "Save the mother and the child, if save thou canst."

I had this point specially in view, and to turn the tide which runs yet among us in favor of craniotomy vs. Cæsarean section. For this reason, and to demonstrate the different steps of the operation, I had invited a limited number of surgeons from different parts of our State to be present, whose professional standing leads me to think that if an indication for abdominal delivery in their respective territories should occur, they would certainly be on hand and utilize the points demonstrated on this occasion. The interest felt by these gentlemen can be best demonstrated by my asserting that every single one of those invited was on hand, though some of them had to travel hundreds of miles. Their zeal and sympathy are a guarantee that they will be coöoperators in a work which is so eminently filled with good, and which must be done—the sooner the better.

CASE.—My patient was a rachitic dwarf, twenty-nine years of age, and of German nationality. Last menstruation August 22d. Quickeening January 10th. She called at my office some time in March, telling me she was *enciente* and requested my attendance at her confinement, for the old women in her neighborhood had told her she would have a hard time. She expected her delivery about the 1st of June. An examination revealed a kyphotic spine with lumbar compensating lordosis, depressing the sacrum to such a degree that it lay almost horizontal; the promontory nearly above the symphysis pubis, distance barely one and three-quarters inches. Transverse diameters elongated and irregularly contracted. This condition of the pelvis called for delivery by Cæsarean section, especially as the arching of the sacrum would have made craniotomy, had it been contemplated, fully as dangerous to the mother, to say nothing of sacrificing the child.

I saw her about a month later, in consultation with my friend, Dr. Park Richie. We both made a careful examination, and came to the same conclusions in regard to diagnosis and treatment. In order to have her in the best possible condition I secured for her a private room in the St. Joseph's Hospital, through the charity of a few ladies, and had her removed there about a week before her expected confinement. I had concluded to induce labor for fear that it might set in just at a time least desirable for us, when some of us had perhaps just been in contact with infectious diseases, and when antiseptic precautions might not be carried out as fully as they ought to be.

Therefore, on the evening of May 28th, I saw her again in consultation with Drs. Richie, Nelson, and McLaren. We all agreed on a conjugate diameter of one and three-quarters inches, and the course to pursue, and concluded by inducing labor by the introduction of an elastic catheter which ruptured the bag of waters and caused their escape. It is needless to say that all antiseptic precautions were taken. She also had taken daily baths, and disinfecting vaginal injections. The catheter was removed and we left her for the night, to await the setting in of labor.

When I called the next morning, shortly after 7 o'clock, I found that she had had sufficient pains during the night. The os uteri was dilated to the size of a silver quarter, and uterine contractions were well pronounced. Her bowels were moved by enema, and she was put in readiness for the operation. Fetal movements were strong, heart-sounds plainly audible to the right of the median line, and the occiput to the right forward.

At 9 A.M., May 29, 1887, I operated assisted by Drs. Richie, Nelson, McLaren, Quinn, Spencer, and Philips, and nurse Frazer, and in presence of a number of invited surgeons. Ether narcosis. Bladder emptied, mons veneris shaved, vagina and abdomen again disinfected. Incision in median line, beginning two inches above the umbilicus and ending one and a half inches above the symphysis pubis. Peritoneum opened in the usual manner. The womb being righted and the abdominal walls firmly pressed against it, I opened it by a few quick sweeps with a scalpel, carefully avoiding the lower uterine segment. The abdominal wound was about nine inches, the uterine about seven inches in length. The child presented with its left shoulder. I extracted it, breech first, without much difficulty, handed it to an assistant, who tied and divided the cord, and who took charge of it. It was a female child, cried at once lustily, weighed seven pounds, and is doing nicely.

The placenta lay to the right, backward. A rubber tube was now passed around the uterus at its upper vaginal junction, the uterus lifted out of the abdominal cavity and a large, warm pad of Lister-gauze put over the abdominal wound to prevent the prolapse of the omentum and bowels, and the entrance of extraneous matter into the abdominal cavity. So far very little blood had been lost, and after the tube was applied there was no more hemorrhage. The placenta and membranes were easily detached and removed, the womb thoroughly mopped out with a 1 to 4000 corrosive sublimate solution, and a quantity, enough to cover thickly the whole endometrium, of equal parts of iodoform and boric acid sprinkled in.

The walls of the womb were now firmly pressed together, and I proceeded to close the wound after Sanger's method, without resecting any muscular tissue or undermining the peritoneum. I used a long, straight, round needle and carbolized silk. The needle was inserted one-third of an inch from the edge of the wound through the peritoneum, through the muscular substance, and out again before reaching the mucosa, then on the opposite side into the muscular tissue above the mucosa, and out through the peritoneal, again one-third of an inch from the edge of the wound. None of the stitches was tied at once, only after all were *in situ*, and a flat sponge removed which had been placed in the cavity of the womb to catch up any blood, were they knotted and cut short. There were eleven deep muscular sutures in all. Next came the sero-serous row of sutures (Lembert-Jobert's) similar to those which we use after resection of the bowels. They were of fine carbolized silk. To apply these I used a half-curved needle in a holder. The needle was

inserted between the deep stitches and over them, just as necessity demanded, and entered about three-eighths of an inch from the edge of the incision line and out again at incision line, then in again at and out three-eighths of an inch from incision line, taking in only the peritoneum. Each thread was knotted as soon as applied, and when all seventeen were *in situ* there was an uninterrupted apposition of serous surfaces covering the muscular stitches and completely closing the uterine gap. The same mixture of iodoform and boric acid was sprinkled over the line of incision, the tube removed and the peritoneal cavity cleaned out, though nothing had escaped into it. A few hypodermatic injections of ergotin and massage brought on good uterine contractions. The womb, which at first, after the removal of the constricting tube, looked rather pale soon assumed a livid color, and when all signs of bleeding had stopped it was returned to its place, and the abdomen closed in the usual manner by eight deep and fourteen superficial silk sutures.

Time from first incision to delivery of child, six minutes.

Time from first incision to application of dressing, thirty-five minutes.

Time given in text-books, one hour and fifteen minutes.

The dressing consisted in iodoform sprinkled over the line of incision, and a large pad of absorbent cotton held in place by a broad binder, the ragged ends of which were tied over the cotton pad. There was no shock, the temperature of the operating-room being above 90° F.

The patient was put in a warm bed, she reacted well, complained of no pain, and would not believe she had been delivered until the child was shown to her.

At noon, temp. 98.4°; pulse 96; cracked ice *ad lib.*; catheter every six hours. At 7 P.M., temp. 98.6°; pulse 108; cracked ice, iced milk, feels comfortable, one-sixth of a grain of morphia subcutaneously.

May 30. 7 A.M., temp. 99.5°; pulse 118; cracked ice, iced milk; lochia normal; ice, wine, and milk. 7 P.M., temp. 99°; pulse 108; cracked ice, iced milk, coffee; lochia normal; comfortable.

31st. 7 A.M., 99°; pulse 108; cracked ice, beef tea; wind passes and urine yet drawn; lochia good; comfortable. 7 P.M., temp. 98.5°; pulse 108; everything favorable; passes water and wind notably.

June 1. 7 A.M., 98.6°; pulse 108; everything favorable; bowels moved by injection; lochia good; milk begins to appear. Beef-tea, milk, coffee, ice, and wine. 7 P.M., temp. 98.6°; pulse 108; comfortable and feels like sitting up.

2d. 7 A.M., temp. 99.4°; pulse 110; restless, lochia scanty, no chill, tympanitic distention of bowels, abdomen tender, vomited once or twice. Enemata and five grains of calomel ordered every four hours; cracked ice and one-quarter of a grain of morphia subcutaneously. At noon Professor Ritchie saw her again with me. Temp. 98.6°; pulse 118; respiration, which up to now had been normal, 28. Ordered one ounce of digitalis infusion with each enema. No movement nor flatus from bowels

yet. Ice-bag to abdomen. At 7 P.M., her condition was much worse. Temp. 104° ; pulse 130; vomiting continues. I gave her another one-quarter of a grain morphia injection, and as her condition kept rapidly getting desperate, I removed the dressing, opened the abdominal wound at its lower third, and introduced a drainage tube, through which I washed out the abdominal cavity with a warm 1 to 4000 corrosive sublimate solution. The water returned just as clear as it was injected.

It was evident we were dealing with a septic process by far too rapid and powerful to control. Her pulse ran up to 150, 160, 180, as far as we could count it. Subcutaneous injections of camphor and ether, and of digitalis had no effect whatever. She became delirious and more and more collapsed. At 10 P.M., being fully aware that she had but a few hours to live, I had ice-bags and ice-cap removed, and as she began to show signs of suffering one-quarter of a grain of morphia was given hypodermically. She died a little after midnight, June 3d.

Autopsy.—June 3d, 9 A.M., Drs. Nelson, Schulin, and Phillips. Examination confined to abdominal organs. Body of a well-nourished, though deformed, woman. Total length three feet ten and one-half inches. Kyphosis of dorsal, lordosis of lumbar region of spine. Legs large, but excessively short. Large panniculus adiposus. Wound in median line of abdomen agglutinated by first intention, except at lower angle, where a rubber drainage tube projected. On opening the abdomen the bowels were found to be distended by gas, but of a natural color; the peritoneum of natural color and consistency throughout, except a small spot in the neighborhood of the left Fallopian tube, which was slightly discolored. No blood, pus, or fluid of any kind in the peritoneal cavity. The uterus well subinvolved, the size of a child's head. The uterine wound perfectly closed and united, every stitch in its place, resisting a considerable strain which was made upon the sutures after the womb had been opened at its posterior portion to test their strength. Thickness of uterine walls three-quarters of an inch. Muscular substance of good color. The endometrium well covered with iodoform, but discolored to the extent of a few square inches, especially in the neighborhood of the left Fallopian tube, where a grayish exudation was present. No bad odor anywhere, but that of iodoform. Ovaries, tubes, etc., healthy. Pelvis generally contracted, conjugate nearly two inches.

Diagnosis.—Endometritis septica (diphtheritica).

Here we have a case, which clinically and pathologically exactly corresponds to one of our old-fashioned childbed fever cases. Especially to that variety where the poison in an overwhelming quantity, so to speak, is conveyed through the lymphatics into the system and where all measures, as curetting the endometrium, intrauterine injections, active catharsis, etc., fail to save women who had just fulfilled their highest and noblest mission.

If we consider how faithfully antisepsis in all its details was carried out in this case it is difficult to

answer the question, how and whence infection took place. And yet we cannot harbor the idea that woman is like a scorpion which carries within its body the poison with which to destroy itself!

The comparatively short space of time which it took to do the operation is noticeable. There was neither hitch nor hurry. For this I am greatly indebted to my corps of often tried and able assistants to whom I take this opportunity to acknowledge my gratitude.

59 IRVINE PARK, ST. PAUL.

AN UNPLEASANT EXPERIENCE WITH THE NEW LOCAL ANÆSTHETIC—HYDROCHLORATE OF ERYTHROPHLEINE.

BY SAMUEL THEOBALD, M.D.,
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CHARITY HOSPITAL.

AFTER reading, with interest, Dr. Alt's article upon hydrochlorate of erythrophleine in the February number of the *American Journal of Ophthalmology*, I procured through Messrs. J. H. Perkins & Co., of Baltimore, a small quantity of the salt made by Merck, wishing to test, for myself, the action of this last-discovered local anæsthetic. On March 23d I used it for the first time, making but a single application of a 1:1000 solution to the eye of a man from whose cornea I wished to remove a foreign body. The anæsthesia which resulted was slow in manifesting itself, was preceded by considerable ciliary irritation, and was not as profound as that which is usually produced by cocaine. The foreign body was removed, however, twenty minutes after the application, without appreciable discomfort to the patient, who was then kept under observation for about an hour longer in order that the subsequent behavior of the eye might be observed.

During this time the pupil of the eye into which the erythrophleine had been instilled became decidedly smaller than the opposite pupil, and accompanying this myosis there was an unmistakable reduction in the intraocular tension. The myosis was by no means as pronounced as that which is induced by even a weak solution of eserine, but the reduction in tension was very marked, and it occurred to me at once that this property of the new agent might make it of value in the treatment of glaucoma. Hirschberg, it was true, had called attention to the fact that rainbow-colors were seen by several patients into whose eyes he had instilled a 1:500 solution of erythrophleine; but, knowing that the seeing of rainbow-colors about a candle-flame does not necessarily indicate high intra-ocular tension, but rather misty media, I felt myself justified two days afterward in trying the effect of the erythrophleine in a case of subacute glaucoma, in which eserine had been used with little effect, and in which I was about to perform an iridectomy.

The patient, a man, forty-nine years of age, and of full habit, was seen at my office. A single application of two drops of a 1 : 1000 solution of erythrophleine hydrochlorate was made to the affected (L.) eye. The condition of the eye before the instillation was as follows, a two-grain solution of eserine having been applied to it the previous night—pupil somewhat contracted; + T. 1; cornea very slightly misty (as seen by oblique illumination), but details of fundus well made out, notwithstanding some post-polar lens opacity; V. (with

Ah. $\frac{1}{4}$ corrected) = $\frac{20}{XLV}$ (?). Within a very few minutes after the instillation of the erythrophleine the eye became more irritable, slight pain was felt in it, and the conjunctival and subconjunctival injection, which previously were insignificant, became much more marked.

At the end of fifty minutes, the symptoms of irritation having increased rather than abated in the interval, the condition of the eye had undergone an astonishing and rather startling change. Instead of the almost imperceptible corneal mistiness previously observed, there was now a very marked cloudiness of the cornea, and apparently of the aqueous humor as well, so that the iris was seen very indistinctly, and the retinal vessels could be distinguished with difficulty. It was thought best not to alarm the patient by testing the sight of the eye, but it could scarcely have been more than one-fourth of what it had been a few minutes before. The tension was probably a little increased, but certainly not markedly so. A subacute glaucoma had, apparently, been converted into an acute attack, and the necessity of an immediate iridectomy suggested itself. I preferred, however, not to operate in the condition the eye was then in, if it could be avoided; so I applied a four-grain solution of eserine sulphate, and, keeping the patient under observation, was gratified to find in about an hour some change for the better in the condition of the eye, the pupil contracting, and the cornea becoming somewhat less misty. Four hours later (*i. e.*, at 4 P. M.), the erythrophleine having been applied at 10 A. M.), there was but little further change in the condition of the eye, and I decided to perform an iridectomy on the following day, a two-grain solution of eserine being directed to be used in the meantime.

The next day the patient reported that he had passed a very uncomfortable night, suffering more pain in the eye than he had ever experienced before; but, since morning, the pain had disappeared, and at the time the iridectomy was performed (2.30 P. M.) the eye had returned to about the condition (as to clearness of cornea, etc.) in which it was before the application of the erythrophleine. The eye did well after the iridectomy, and the subsequent progress of the case presented nothing of an unusual character or worthy of note.

I have not been disposed to experiment very freely with erythrophleine since meeting with the experience which I have related. In several cases of granular lids with pannus, however, which had been

treated with nitrate of silver, preceded by instillation of cocaine (4 per cent.), I substituted for the latter one instillation of erythrophleine (1 : 1000), and in every instance the patient subsequently reported that the eyes felt worse, and that the pain experienced was more severe and more persistent than when the cocaine was used.

PERNICIOUS ANÆMIA WITH JAUNDICE.

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THE following case of pernicious anæmia, though imperfectly recorded, is deemed of interest; first, because it occurred in a colored man, and in the Southern States; second, because the case throws some light on the question of the relations between jaundice and pernicious anæmia.

George S., twenty-eight years old, was admitted into the Charleston City Hospital on January 25, 1888. He was a mulatto, with a large frame, and excellent muscular development.

On the 12th of last November he had been admitted to the surgical ward for the treatment of a large serpiginous ulcer, involving the right groin and portion of the scrotum. He refused treatment, and was discharged on the 24th. All that was noticed, at the time, about the man, beside the ulcer, was his stalwart form.

He was readmitted on January 25th, complaining of weakness, dyspnea and cough. He stated that he had been feeling weak since last fall. He had been, however, working as a mason up to two weeks before admission. The diagnosis on his card for admission was pulmonary consumption. I found him very anæmic, but not emaciated. A physical examination of the chest proved that there was no tuberculosis, but a general emphysema of the lungs. The serpiginous ulcer was still present, though of smaller size. I ordered, therefore, the patient to be transferred to the surgical ward on the 27th. By this time he had had several paroxysms of dyspnea, and I was asked to examine the patient again. I found a marked reduplication of the first sound. This symptom, taken in connection with the fever, the history of suppuration and the peculiar paroxysms, led me to diagnosticate infectious endocarditis. Under this erroneous diagnosis I received the patient once more in the medical wards on January 31st.

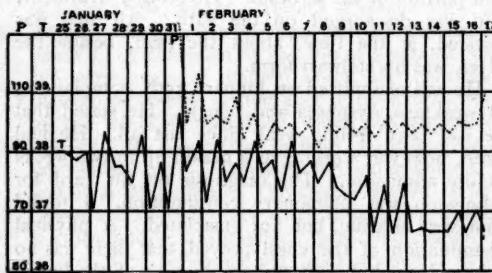
The following symptoms were observed: The mucous membranes were blanched, but the striking appearance of pernicious anæmia was masked by the color of the patient. The conjunctivæ were distinctly yellow. The skin of the front of the legs was scaly and purplish, as if marked by traces of chronic eczema. The eyelids were always more or less puffy, and the ankles swelled slightly when the patient sat up any length of time. This he was inclined to do in spite of orders to the contrary. His mood, in fact, was often buoyant, and some-

times flighty. He was easily tired, however, and would take to his bed.

He took very little food of any kind, and vomited very often. The vomiting was never spontaneous. The matters vomited consisted of food, and a frothy chyme of a light yellow color. There was no diarrhoea.

The patient had much cough at the time of admission, but this symptom subsided gradually. He complained of a constant uneasiness about the precordia, which was aggravated, at times, by paroxysms of precordial pain with suffocation. These occurred more frequently at night. The pulmonary resonance was exaggerated throughout the chest, and the emphysematous lung encroached upon the areas of cardiac, hepatic and splenic dulness. It was possible, however, to discern some enlargement of the left side of the heart. The heart sounds over the body of the organ presented a well-marked galloping rhythm. The intervening sound became so friction-like toward the last, that I concluded there must be some pericardial roughness.

The urine was examined soon after admission by the Resident Physician, Dr. A. H. Hill. It was found to contain biliary coloring matter, but no albumin and no sugar. The liver was found to be pushed down by the emphysematous lung. There was some enlargement of the liver and spleen.



Temperature chart. Pernicious anæmia.

On the 14th of January the patient began to appear dull and drowsy. The icterus became more marked, and the oedema of the face more pronounced. The swelling now produced considerable distortion of the features, the right side of the face being more swollen than the left on account of the decubitus. There was very little general oedema.

On June 17th the patient was comatose, and had been so since the previous evening. The inspirations were remarkably deep and regular. The heart beat 110 to the minute, but no pulse could be felt at the wrist.

A drop of blood was obtained this morning with difficulty, by sticking and forcibly squeezing the finger. The fluid had a reddish amber color. On microscopic examination the oligocythaemia was found to be extreme. Though the quantity of blood under the cover glass was sufficient completely to fill the space, yet the red corpuscles were found isolated and sparingly scattered. There was no marked poikilocytosis. In several fields no leucocytes could be found. Instead of making a more

careful examination of the blood I satisfied myself with the explanation that some serum from the subcutaneous connective tissue had been pressed out with the blood.

The patient died on the evening of the 17th, without convulsions. The treatment consisted principally in the administration of gtt. x of tincture of the chloride of iron every three hours.

Autopsy twelve hours after death. There was some purplish discoloration of the skin on the anterior surface of the legs. The oedema was scarcely noticeable except about the face. The *panniculus adiposus* was normally distributed, and the color of the muscles was pale. These tissues were bloodless.

The lungs were found abnormally distended, covering much of the pericardium, though this sac was large. The pleural cavities contained a small amount of serous effusion stained yellow. The lungs, after removal, preserved their form and bulk, as if they were inflated. Near the anterior border of the left lung there was a small bleb of vesicular emphysema. The rest of the lungs was in a condition of universal lobar emphysema. There was no tuberclosis. A yellowish, frothy fluid could be expressed from the anaemic lungs. The pericardium contained about six ounces of serum decidedly stained with yellow. The visceral layer of the membrane presented a patch of chronic thickening upon the body of the right ventricle. There was no excess of fatty deposit. The heart was somewhat enlarged, especially the left ventricle, which was firmly contracted. The muscular tissue was pale but not friable. The ventricles contained each a small lump of yellowish clot, tangled with the cords, but no fluid blood. The mitral valves were somewhat enlarged and thickened at the edges. There was some thickening and opacity of the endocardium toward the apices of the papillary muscles. The walls of the large bloodvessels were stained yellow.

The peritoneum was blanched. The intestines, below the duodenum, were remarkably contracted, as they appear below the seat of an intestinal obstruction; the stomach and duodenum were comparatively dilated. There was no effusion into the peritoneal cavity. The peritoneum contained no excess of fat. The liver was somewhat enlarged; perhaps not indeed increased in volume, but flattened out, especially along the anterior border. The capsule was smooth and transparent. The color of the organ was dark bile-green. The section surface was smooth, and the parenchyma was uniformly infiltrated with bile. The acini were not distinct. The gall-bladder was half filled with bile, which was of a lighter color, and more fluid, perhaps, than usual. The biliary passages were not obstructed. The mucous chyme contained in the small intestine was evidently mixed with bile, being of the same color as the secretion in the gall-bladder. The mucous membrane was stained in the same manner. The stomach contained a small amount of pale yellowish fluid. The rugæ were not as prominent as usual, but the walls did not appear to be atrophied. The mucous membrane had a fatty appearance, and it was studded with ecchymotic spots to the number of one to the square inch. These had a

roundish outline, and measured from the size of a pin-head to that of a small pea. They were situated superficially, and some of them had been loosened, apparently by post-mortem digestion. The intestines were carefully searched for parasites, but none was found. The pancreas was somewhat enlarged and fatty.

The spleen was one-half larger than the usual size. The color was dark-purplish, and the consistence was firm. A considerable number of fine white connective-tissue trabeculae were visible. The Malpighian bodies were not apparent. The kidneys were slightly enlarged, very pale and firmer than in health. The capsule was not adherent.

The medulla of the humerus, the radius and the ulna presented the same color as the spleen.

REMARKS.—My impression is that very few cases of pernicious anæmia have been reported from the South. Looking over the *Index Medicus* I find no case reported in any of the Southern medical journals, unless the two following be exceptions: W. Lee, a case of anæmia with excessive irritability of the stomach treated successfully by the hypodermic use of ammonio-citrate of iron (*Maryland Medical Journal*, iv., 1878); and Miller, C. H., a case in search of a diagnosis (*Southern Clinic*, iv., 1881).

I confess that the error of diagnosis in this case has shaken my opinion concerning the rarity of the disease in the southern section of the country, for it has brought forcibly to me the possibility of being misled by the pigmented skin of the negro. The anæmias of this section, and of the tropics, certainly require extended investigation. The recent writers on tropical diseases of America do not mention, as far as I know, the special form of anæmia now under consideration, though they insist upon the pernicious tendencies of the paludal anæmia. One is inclined to suspect that cases of Biermer's disease may have been included in this category; but, it must be stated that the accounts given by students of tropical pathology do not coincide with what we still have to hold as a distinct disease under the description of Addison and Biermer. One case of pernicious anæmia, I find, has been reported from Havana, by Dr. Rijó, in the *Cronica Medico Quirurgica*.

Judging from my own experience, and again from the records of the *Index Medicus*, it appears that Hodgkin's disease is comparatively common in the South, and leukæmia very rare.

In regard to the relations of jaundice and pernicious anæmia, I must refer the reader to an interesting paper of F. E. Georgi, entitled "Gallensteine und perniciöse Anæmie" which appears in the *Berliner klinische Wochenschrift* of October 31st and November 7, 1887. The author reports a case of pernicious anæmia, in which, he contends, the first symptoms to present themselves were those of hepatic

disturbance. The post-mortem examination revealed the presence of gall-stones. After referring to eight cases of pernicious anæmia with jaundice, he concludes that, in such cases, the retained biliary secretion is the essential cause of the anæmia, by inducing solution of the haemoglobin. This opinion is criticised in the last of the two numbers just quoted, of the *Wochenschrift*, by C. A. Ewald, who contends that the jaundice is secondary to the anæmia. He maintains that the jaundice in question is not obstructive, but the result of rapid disintegration of red blood-cells. The consequence of this is excessive production of biliary products, inability of the liver and the digestive functions to dispose of this excess, and the reabsorption of the latter from the liver into the circulation. My own case gives very decided support to the latter view. The jaundice in this case was certainly not obstructive. The same may be said of the second of the two cases reported by F. P. Kinnicutt to the Association of American Physicians (*American Journal of the Medical Sciences*, October, 1887).

CHARLESTON, May, 1888.

FRACTURE OF THE STERNUM, WITH DISLOCATION OF THE FRAGMENTS: TREATMENT AND RESULT.

BY B. J. D. IRWIN, M.D.,
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COLONEL U. S. ARMY.

Two cases of fracture of the sternum having been recently reported in the *Boston Medical and Surgical Journal*, vol. cxviii. pp. 368, 369, the writer is induced to place on record, from notes taken at the date of the occurrence, another instance of this rare form of injury.

While on duty at the United States Military Academy, in the summer of 1876, I was called in consultation by Dr. J. B. Monroe, of Highland Falls, near West Point, New York, to see John Turner, a healthy, robust American, aged twenty-six years, who, while seated on top of an omnibus, was dragged by his unruly team through a low doorway of a carriage-house, his body being caught between the lintel and the driver's seat, and crushed violently backward and downward against the roof of the vehicle. As he was hurried forward toward the entrance he endeavored to escape the danger from above by stooping and leaning forward, but his body was caught by the lintel and violently crushed backward, his dorsal spine being pressed against the projecting comb of the seat, formed on the roof of the omnibus. At the same time the compression of the thorax fractured the sternum at the junction of the manubrium with the gladiolus, the lower end of the former being driven inward, while the upper extremity of the latter bone was pushed outward and upward upon the manubrium.

Attempts to return the fragments to their normal position having proved unsuccessful, a broad bandage

had been applied for the purpose of lessening the intense pain caused by thoracic respiration.

Examination of the injured parts disclosed marked projection of the spinal column between the second and sixth dorsal vertebrae, but, although the appearance of the parts indicated fractured bone, careful manipulation failed to disclose crepitus.

Efforts were at once made to replace the fragments of the broken sternum, but, although they could be reduced to position by pressing the overlapping, projecting end of the gladiolus downward, it would slip out and upward on withdrawal of the direct pressure.

To overcome that tendency, a large, hard pad, covered with soft material, was placed between the scapulae. While the patient inspired deeply and the head and shoulders were extended well back, the displaced fragments were returned and retained in their normal position by a suitable splint applied over the pad and across the spine; the expanded condition of the thorax being maintained by aid of a figure-eight bandage, which bound the shoulders back to the projecting ends of the transverse splint.

To allay pain, which was very severe during the two or three days following the reception of the injury, morphine and potassium bromide were administered from time to time, as occasion required.

The apparatus was worn without change for three weeks. Its use secured perfect reunion of the broken bone and complete restoration of the sternum to its natural condition.

SAN FRANCISCO, CAL., May, 1888.

MEDICAL PROGRESS.

Myxedema.—DR. ORD, as Chairman of the Myxedema Committee of the Clinical Society of London, at the last meeting of the Society, presented its report, of which the following are the conclusions:

1. That myxedema is a well-defined disease.
2. That the disease affects women much more frequently than men, and that the subjects are, for the most part, of middle age.
3. That clinical and pathological observations respectively indicate, in a decisive way, that the one condition common to all cases is destructive change of the thyroid gland.
4. That the most common form of destructive change of the thyroid gland consists in the substitution of a delicate fibrous tissue for the proper glandular structure.
5. That interstitial development of fibrous tissue is also observed very frequently in the skin, and with much less frequency in the viscera, the appearances presented by this tissue being suggestive of an irritative or inflammatory process.
6. That pathological observation, while showing cause for the changes in the skin during life, for the falling off of the hair, and the loss of the teeth, and for the increased bulk of the body, as due to the excess of subcutaneous fat, affords no explanation of the affections of speech, movement, sensation, consciousness and intellect, which form a large part of the symptoms of the disease.
7. That chemical examination of the comparatively few available cases fails to show the general existence of

an excess of mucin in the tissues adequately corresponding to the amount recorded in the first observations, but that this discrepancy may be, in part, attributed to the fact that tumefaction of the integuments, although generally characteristic of myxedema, varies considerably throughout the course of the disease, and often disappears shortly before death.

8. That in experiments made upon animals, particularly on monkeys, symptoms resembling in a very close and remarkable way those of myxedema, have followed complete removal of the thyroid gland, performed under antiseptic precautions, and with, as far as could be ascertained, no injury to the adjacent nerves or to the trachea.

9. That in such experimental cases a large excess of mucin has been found to be present in the skin, fibrous tissues, blood and salivary glands; in particular, the parotid gland, normally containing no mucin, has presented that substance in quantities corresponding to what would be ordinarily found in the submaxillary gland.

10. That the full analysis of the results of the removal of the thyroid gland in man demonstrates in an important proportion of the cases the fact of the subsequent development of symptoms exactly corresponding to those of myxedema.

11. That in no inconsiderable number of cases the operation has not been followed by such symptoms, the apparent immunity being, in many cases, probably due to the presence and subsequent development of accessory thyroid glands, or accidentally incomplete removal, or to insufficiently long observation of the patients after operation.

12. That, whereas injury to the trachea, atrophy of the trachea, injury of the recurrent laryngeal nerves, injury of the cervical sympathetic and endemic influences have been, by various observers, supposed to be the real causes of experimental or of operative myxedema (cachexia strumipriva), there is, in the first place, no evidence to show that, of the numerous and various surgical operations performed on the neck and throat, involving various organs and tissues, none, save those in which the thyroid gland has been removed, has been followed by the symptoms under consideration; that in many of the operations on men, and in most, if not all, of the experimental operations made by Professor Horsley on monkeys and other animals, the procedure avoided all injury of surrounding parts, and was perfectly aseptic; that myxedema has followed removal of the thyroid gland in persons neither living in, nor having lived in, localities the seat of endemic cretinism; that, therefore, the positive evidence on this point outweighs vastly the negative, and that it appears strongly proved that myxedema is frequently produced by the removal, as well as by the pathological destruction, of the thyroid gland.

13. That whereas, according to clause 2, in myxedema women are much more numerously affected than men, in the operative form of myxedema no important difference of the same kind is observed.

14. That a general review of symptoms and pathology leads to the belief that the disease described under the name of myxedema, as observed in adults, is practically the same disease as that named sporadic cretinism when affecting children; that myxedema is probably identical with cachexia strumipriva; and that a very close affinity exists between myxedema and endemic cretinism.

15. That, while these several conditions appear, in the

main, to depend on, or to be associated with, destruction or loss of the function of the thyroid gland, the ultimate cause of such destruction or loss is at present not evident.

—*Lancet*, June 2, 1888.

Digitalis as a Diuretic and Laxative.—HUCHARD, in the *Revue Gén. de Clin. et de Thér.* of April 12, 1888, gives the following formula for a laxative powder containing digitalis:

Potass. sulphat,	
Potass. tartrat,	
Potass. nitrat.	aa gr. 90.
Fol. digital. (pulv.)	gr. 15.

In twenty powders.

One powder three times daily.

The Use of Gray Oil in Syphilis.—At a late meeting of the Society of Physicians at Vienna, PROF. LANG recommended the use of "gray oil" in syphilis. Recognizing the powerful curative properties of metallic mercury as it exists in blue ointment, the subcutaneous injection suggested itself. The preparation which he has used for four years consists of

Mercury,	
Lanolin	aa 3 parts.
Olive oil	4 parts.

As to the quantity of "gray oil"—as Lang entitles his preparation—to be injected, experience teaches that very small amounts have a powerful influence upon the syphilitic process. Moderately severe forms of syphilis require hardly more than five minimis of gray oil a week. As a rule, one and a half to two minimis of "gray oil" are injected into two places (buttocks or back) every five to eight days. In two or three weeks, most of the symptoms will have ameliorated or entirely disappeared, and the quantity can then be reduced. As the influence of the injections persists, treatment may be intermittent for ten to fourteen days. Then, the injections are resumed, a quantity equal to that first used being injected at longer intervals, or smaller quantities at like intervals, until twenty-four to thirty-two minimis have been injected. Lang advises watching for affections of the mouth and other signs of acute mercurialism.

Knowing the technique, securing a faultless preparation of the remedy and observing antiseptic principles, good results are to be obtained almost without exception. The remedy is well borne; in the four years in which Lang has used "gray oil," only in four or five cases has he been compelled to withdraw the treatment, either because the patient could not bear injections or because an idiosyncrasy to mercury existed.

The inferences from his experience are:

1. That in "gray oil," the mercury is equably distributed, and the dosage can be accurately determined.
2. Convenience of the application.
3. Slight reaction. In none of the many cases thus treated by Lang did suppuration occur at the site of injection.
4. The manifest advantage of the method, however, is that in "gray oil" the mercury is contained in a form similar to that in which it exists in blue ointment.

The treatment with "gray oil" is indicated in all cases in which general treatment with mercury is indicated.

There is no method more efficient, or whose results would be more permanent. The best test for a remedy for syphilis is in the success attained in syphilis of the central nervous system, and it is in doubtful cases of syphilis of the brain and spinal cord that Lang obtained very satisfactory results.

An important point is that "gray oil" can be used in cases in which blue ointment, or mercurial plaster cannot be applied. Thus, the author injected into a gummatus cavity in the tibia with a small external opening "gray oil," and the cavity closed. "Gray oil" is also adapted to the local treatment of syphilitic processes in the larynx, in the nose, of the tympanum and of the eye.

In reply to inquiries, Lang stated that immediately after the injection no pain is observed; on the following day, a small infiltrate is apparent, with sensitiveness, which lasts two to five days; after six to eight days, the site of injection is no longer discernible. Comparative experiments with injections of calomel showed that "gray oil" occasioned milder subjective and objective manifestations than calomel. Lang has not investigated how long the mercury remains in the organism, but it has not been demonstrated that remedies which are speedily eliminated are the less active. He has not used "gray oil" in hereditary syphilis. The remedy is not to be advised for children, because they bear subcutaneous injections poorly; but in the case of adults it ought to be useful.—*Wiener medizin. Presse*, May 20, 1888.

Intestinal Occlusion in Pernicious Fever.—DR. A. W. REYES, of Sagua la Grande, Cuba, in an instructive paper on rare forms of pernicious fevers, directs attention to the occasional sudden occurrence of intestinal occlusion due to violent tonic spasm of the intestinal muscular fibres, which is sometimes associated with spasm of voluntary muscles also. In these cases there is great difficulty in arriving at a correct diagnosis, for the temperature is usually only slightly above the normal, the extremely prostrate, sometimes cyanotic, condition of the patient coming on very suddenly, and there being nothing to point to the malarial origin of the affection. Nevertheless, the rapid and powerful effect of quinine, especially if administered hypodermatically, together with the exhibition of some alcoholic stimulant by the mouth, is, he considers, a pretty sure indication of the true nature of these cases.—*Lancet*, May 12, 1888.

Chloroform Water as a Vehicle.—UNNA, in *Monatshefte für Praktische Dermatologie*, No. 9, 1888, writes that he has found chloroform water an excellent vehicle for the hypodermatic administration of Fowler's solution and ergotin, as it prevents the decomposition of the solution. It is also useful in giving morphia where a local anæsthetic and counter-irritant effect is desired, as in neuralgias. It is equally valuable in the internal administration of drugs which decompose rapidly. Atropine and bichloride of mercury may also be given in chloroform water.

Alanin-mercury Hypodermatically in Syphilis.—PROFESSOR DE LUCCA, of Catania, has found that a combination of mercury with amido-propionic acid, or alanin, as it is called, is preferable to all other mercurial preparations for hypodermatic use in syphilitic cases. Alanin-mercury

is thus prepared: One part of alanin is dissolved in twenty parts of water by heating gradually until it boils. While the solution is boiling binoxide of mercury is added little by little until no more will dissolve. The whole is then filtered and evaporated. The alanin-mercury crystallizes out in minute needles of a dull white color. With this substance Professor de Lucca has treated forty cases of recent syphilis. A very small dose is sufficient, the average daily quantity, given hypodermatically, being $\frac{1}{16}$ th of a grain dissolved in 15 minims of water. In very few cases were abscesses produced. Alanin-mercury seems to be peculiarly suitable for internal use in infantile syphilis. In these cases from $\frac{1}{16}$ th to $\frac{1}{8}$ th of a grain was given daily, and in no instance was there any sign that it could not be borne. Professor de Lucca states that the cures produced by alanin-mercury appeared to be of a permanent character.—*Lancet*, April 28, 1888.

Naphthalin in place of Arsenic as an Insecticide.—HAGER calls attention to the dangers attending the use of arsenic and its preparations as an anti-parasite and vermin-killer. He strongly recommends to use naphthalin whenever arsenic would be indicated for the above purposes. He gives among others the following formula:

Naphthalin	40 parts.
Chloroform	80 "
Benzine	100 "
Mix.	

Heat carefully to about 20° C., and agitate till the naphthalin is dissolved. American petroleum may be substituted for the benzine. These solutions may be applied as they are to the skin of animals infested with vermin, or they may be applied in a diluted state. The naphthalin may be incorporated with vaseline if an ointment is desired for use, or if an oily liquid is wanted the above solutions may be mixed with liquid vaseline in the proportion of 200 of the former to 1200 of the latter. Hager also speaks highly of the infusion of quassia as an anti-parasitic application.—*Pharmaceutical Record*, May 15, 1888.

Guaiacol in Phthisis.—HORNER, in the *Prager med. Wochenschrift*, No. 17, 1888, reports that he has used guaiacol in phthisis for four years. The dose has been $7\frac{1}{2}$ grains, in pill form, beginning with three pills and increasing to ten. General improvement followed in the greater number of cases. The tubercle bacilli became less in number.

Changes in the Oxyhemoglobin of the Blood Produced by Typhoid Fever.—HÉNOCQUE and BANDONIN have made daily spectroscopic examinations of the blood in typhoid fever, finding that the lowest point in the course of oxidation was reached about the eighth day. The occurrence of diarrhoea and of pulmonary complications reduced the consumption of oxygen by the blood and other tissues. In general, oxidation in the body was in inverse proportion to the elevation of temperature and severity of symptoms.—*Revue Gén. de Clin. et de Thér.*, May 3, 1888.

Camphorated Carbolic Acid.—SCHNEIDER, a Swiss physician, recommends camphorated carbolic acid as an "elegant, reliable, and very convenient antiseptic pre-

paration." As is well known, when one part of crystallized carbolic acid and three parts of powdered camphor are shaken up together in a test-tube, a colorless limpid fluid is produced. This mixture does not possess either the characteristic odor or the rubefacient and caustic properties of carbolic acid, while the antiseptic power of the latter remains intact. When placed on the tongue the compound causes but a very slight burning sensation. It has no effect on polished steel.—*British Medical Journal*, May 12, 1888.

An Application for Hysteria of the Larynx.—The *Journal de Médecine de Paris* of May 13, 1888, gives the following formula:

Cocain. hydrochlor.	gr. $\frac{1}{10}$ to $1\frac{1}{2}$.
Aqua destill.	3 $1\frac{1}{2}$.
Potass. chlorat.	gr. $\frac{1}{4}$.
Aq. amygdal. amar.	m. 1.

For local application in laryngeal hysteria.

Atropine in Pilocarpine Poisoning.—DR. WICHERKIEWICZ records in a Polish medical journal a case of poisoning by pilocarpine. Eight minims of a two per cent. solution of pilocarpine had been administered hypodermically in the temporal region for an ophthalmic affection. This was followed by very severe symptoms of poisoning and collapse. A subcutaneous injection of morphia and the inhalation of nitrite of amyl proving useless, two drops of a one per cent. atropine solution were administered hypodermically. This had more effect, and the patient recovered.—*Lancet*, April 28, 1888.

The Germs Contained in Vaccine Lymph.—PFEIFFER is quoted by the *Centralblatt für Bakteriologie*, No. 19, 1888, in his report of examinations of vaccine lymph to determine the germs contained in it. He found that an absolutely pure lymph is only rarely obtained; the germs most frequently found are spores, rarely in lymph from children, constantly in vaccine lymph. Sarcinae are also found in both human and vaccine lymph, they are of no practical importance. Bacteria and bacilli are found exceptionally in human lymph, more frequently in vaccine matter. Micrococci are found, the most constant being *staphylococcus cereus albus* and an orange-colored micrococcus identical with the *pyogenes aureus*. Pathogenic micrococci occur frequently, most often the *staphylococcus pyogenes aureus, albus* and *citreus*; streptococci, those of erysipelas and the *pyogenes* are not found in animal lymph. The bacillus of syphilis does not flourish in bovine lymph and hence syphilitic contagion cannot be obtained from this source. The bacilli of tuberculosis may, however, be inoculated in vaccine lymph.

Imitation Kefir.—A very good and agreeable imitation of the Tartar beverage known as "kefir," which is, like koumiss, used extensively in phthisis and other wasting diseases, may be made by the following simple method described by Dr. Levy in a German chemical journal: Freshly prepared sour milk is briskly shaken up and then placed in a soda-water bottle, together with two per cent. of syrup. The mixture is well corked and kept in a warm place for three or four days. At the end of that time a most agreeable effervescent beverage is obtained

by uncorking the bottle. It contains some two per cent. of alcohol. If required for use more speedily, a few drops of lemon juice should be added to the syrup.—*Lancet*, April 28, 1888.

A Convenient Formula for the Treatment of Tapeworm.—Oil of male fern may be conveniently administered in the following combination:

Ethereal oil of male fern	ml. 45.
Tinct. vanillæ	ml. 45.
Syrup rubi.	3 6 1/4.
Gum. acac. pulv.	gr. 30.
Aquaæ destill.	3 6 1/4.

To be taken at one dose, in an equal quantity of milk. Castor oil should be taken two hours afterward.—*L'Union Médicale*, May 10, 1888.

The Histology of Peripheral Neuritis.—A new contribution to the histology of peripheral neuritis has been made by PROF. EICHORST, of Zurich (*Virchow's Archiv*, cxii. Hft. 2). The case he describes is that of an habitual drunkard, who died from alcoholic paralysis after six weeks' illness. The symptoms began with incoordination and then paralysis of the legs, followed by paralysis of the extensors of the wrist, with rapid wasting of the paralyzed muscles, tenderness of the muscles, some cutaneous anaesthesia, abolition of reflexes, and ultimately vesical and rectal paralysis. At the post-mortem examination, no gross lesions could be seen in the nerve centres. The brain was not examined microscopically, but the cord showed some asymmetry in the anterior cornua, and some recent hemorrhages in the gray matter in the dorsal region, with thickening of the bloodvessels—changes which did not suffice to explain the general paralysis. The spinal nerve roots were found to be normal. The tibial and radial nerves, on the other hand, were found to be profoundly diseased, osmic acid preparations showing very extensive degeneration and atrophy, with absence of the axis-cylinders in a large proportion of the nerve fibres. Tracing the nerves to their peripheral terminations in the muscles, Professor Eichorst found the lesions to be more advanced and complete there than in the nerve trunks. Indeed, within a muscle there was not a single normal nerve fibre. Nor were the lesions of these intra-muscular nerves limited to degeneration, as in the nerve trunks; but each degenerated fibre was surrounded by numerous laminae of connective tissue produced from the endoneurium and perineurium. It would seem as if the existence of the degenerate nerve substance had excited inflammatory change around it. Another remarkable fact was that the muscular fibres did not exhibit the trophic changes usually met with in neuritis, although in the vicinity of the diseased nerve-endings the muscular fibres were atrophied apparently secondary to the perineurial lesion. Professor Eichorst therefore proposes to term the condition "neuritis fascians," as denoting what he considers to be the essential feature of the change; viz., the inflammation of the nerve sheath, and the extension therefrom to the interstitial tissue of the muscles. He points out that similar changes in muscles have been described by Fränkel in phthisis, and by Eisenlohr in infantile paralysis; the former raising the question whether the ensheathing bands of connective

tissue arose from the bloodvessels or the neurilemma. Eichorst does not infer that neuritis fascians is special to alcoholic paralysis, but that it is an important feature of it. Certainly the clinical phenomena of "peripheral neuritis" harmonize with the occurrence of lesions within the muscles themselves, and we may direct the attention of pathologists to the more precise determination of such lesions, which in all probability precede the degeneration of fibres observed in the nerve trunks. The latter change has been amply studied, but the former, and indeed the essential change, has not as yet been much dealt with.—*Lancet*, May 19, 1888.

Antipyrin as a Haemostatic.—In view of the statements of HENOCQUE and HUCHARD as to the haemostatic properties of antipyrin, DR. ALEXEI G. GLINSKY, of Kharkov, tried (*Transactions of the Kharkov Medical Society*, Part I., 1887, p. 23) the drug (in the shape of cotton-wool plugs soaked in a five or ten per cent. solution, or powdered with the antipyrin) in a series of cases of bleeding, such as epistaxis, hemorrhage from surgical or accidental wounds, etc. On the whole, his results were unsatisfactory. In some cases of lacerated wounds of the fingers (inflicted by nail, etc.) hemorrhage was arrested, but it was not certain that this effect was not rather due to pressure than to the antipyrin. Dr. Glinsky tried it on a wound on his own finger, but was obliged to have recourse to perchloride of iron. In three cases of severe epistaxis the bleeding came on again in five or ten minutes after withdrawing the antipyrin plugs, and ultimately could be stopped only by plugging the posterior nares. Antipyrin had no effect in checking hemorrhage after excision of acute condylomata, labial papillomata, etc. On the other hand, Dr. Glinsky was much pleased with antipyrin as a dry dressing in indolent ulcers of the leg, fully confirming Dr. Boss's observations (*Berliner klin. Wochenschrift*, No. 33, 1886) on this point. The healing action of the drug in these cases is so rapid that he strongly recommends the use of antipyrin in powder when other means fail. He also obtained good results with antipyrin in acute articular rheumatism, in which it is said to act as effectively as salicylate of soda and more rapidly. It also proved useful in migraine and neuralgia of the fifth nerve. Unpleasant secondary effects (rash, sickness, collapse) occurred in his experience very rarely.—*British Medical Journal*, May 19, 1888.

A Simple Apparatus for Sterilizing Instruments.—DR BACKER describes, in the *Journal de Médecine* of May 6, 1888, a simple apparatus, consisting of a rectangular water-bath hermetically sealed containing a pound of paraffine, ordinarily melting at 119° F., and which can be heated to 572° F. without volatilizing. In the water-bath is placed a flask containing the articles to be sterilized. Heat is furnished by an alcohol lamp.

Skin Grafting with Cock's Comb.—SEÑOR F. ALTRAMIRANO mentions in a Mexican journal, *El Observador Médico*, a case in which he made some experiments in skin grafting on an obstinate ulcer left by a large carbuncle. More ordinary means having failed to cause it to heal, he applied three skin grafts taken from the patient himself. Of these only one took, and the man expressed so much repugnance to the proceedings that another source had to be found for subsequent grafts. A

cock was selected, and fragments cut from his gills; these were split, divided into ten pieces, and the raw surfaces applied to a freshened portion of the ulcer. A carbolized dressing was then bandaged on, the whole being constantly moistened. On the third day, on the dressing being removed, all the ten fragments were found organically adherent. A number of new grafts were then applied, cut from the comb, the gills, and the skin of the back of a chicken. None of these was successful. The cock's gills were again resorted to, and four more fragments successfully grafted. A fresh set of grafts were prepared from this source; but all the cellular tissue was cut or scraped away, in some cases little but the epidermis being left, in others the edges being bevelled, so that the epidermis was in direct contact with the new surface of the ulcer. This set of grafts proved very unsuccessful, so that Dr. Altramirano was led to the conclusion that no advantage is gained by applying the epidermic layer immediately in contact with the surface of the ulcer.—*Lancet*, May 19, 1888.

The Treatment of Gall-stones.—ROSENBERG, at a recent meeting of the Berlin Medical Society, stated that upon an American suggestion he had treated with large quantities of olive oil a patient, who, for five years had suffered with attacks of nephritic colic, and had in vain used the most various remedies. The patient took on several evenings, without marked discomfort, from 3 to $5\frac{1}{4}$ ounces of olive oil, followed by some Cognac, and after the first dose passed 3 calculi, after the second, 243. In all, $26\frac{1}{2}$ ounces of olive oil were taken, in five doses, and 629 calculi counted in the stools; subsequently to which the gall-bladder, which had previously extended markedly beyond the margin of the liver, could no longer be palpated. According to the views of American physicians, the usefulness of the olive oil depends upon its entrance into the gall-bladder and softening the calculi.—*Wien. medisin. Presse*, May 20, 1888.

The Influence of Antiseptics on the Kidneys.—At the last meeting of the Berliner Medicinische Gesellschaft, DR. EMIL SENGER read a paper on the influence of antiseptic remedies on the organs of the body, with special reference to operations on the kidney. It is well known that after nephrectomy, or even nephrotomy, many patients die with symptoms of uremia or anuria, even when it had been ascertained beforehand by careful examination that the other kidney was quite healthy and capable of secreting the necessary amount of urea. Dr. James Israël, chief surgeon of the Berlin Jewish Hospital, has propounded a very complicated theory as to certain nervous sympathies between the two kidneys, whereby an operation on one may give rise to degeneration of the other. Senger has now proved by experiments on rabbits and dogs that our antiseptic remedies are the cause of these complications. He injected into the animals, when in perfect health, one-tenth or twelfth part of the quantity of corrosive sublimate, carbolic acid, etc., which is sufficient to kill them. He then extirpated one kidney, and examined it microscopically, with the result that in all cases he found glomerulo-nephritis. There was exudation between the glomerulus and the capsule, and the epithelium of the tubuli contorti was almost entirely destroyed. He found also fatty degener-

ation of the liver, the spleen, the heart muscle, etc. The various antiseptic agents were found to be injurious in different degrees, corrosive sublimate being the most dangerous, then the others in the following order: iodiform, carbolic acid, salicylic acid, boric acid. Senger therefore recommends surgeons to avoid antiseptics in operations on the thorax and abdomen, and urges them either to employ sterilized water after the manner of Mr. Lawson Tait, or a solution of salt. By bacteriological and pathological researches he proved, first, that this kills the streptococcus pyogenes aureus in twenty-eight minutes, and that the effect is independent of the degree of concentration, for a five per cent. solution is just as effectual as a twenty per cent. Secondly, he claims to have shown that chlorate of sodium does not in any way injure the organs, and that no dose is strong enough to kill any animal.—*British Medical Journal*, May 19, 1888.

A Case of Ectocardia Relieved by Operation.—LANNE-LONGUE recently reported to the Academy of Sciences of Paris, the case of a female infant, six days old, brought to his care at the hospital Trousseau, with congenital absence of the anterior wall of the thorax, resulting in an ulcerated aperture as large as a ten cent piece. The day following admission to the hospital, a yellowish membrane which had partially closed the aperture, was removed by ulceration; the anterior surface of the ventricles was visible, and the pericardium was seen to be absent. Progressive ulceration of the borders of the opening, with retraction of the tissues, was gradually forcing the ventricle and apex of the heart forward and without the thorax. A plastic operation was made by loosening flaps on each side of the aperture, and uniting them by sutures, leaving their bases attached for blood supply. No blood was lost by the operation, and cicatrization resulted in less than three weeks. Two months and a half after operation the child was in comfortable health, and it was probable that as the heart increased in size, it would take its normal position within the thoracic walls.—*Gazette Hebdomadaire*, May 11, 1888.

Cresote Hypodermatically, in Phthisis.—In a recent work on the treatment of phthisis by HÈRARD, CORNIL and HANAT, the following formula is given:

Peptone, dried	3 2 1/2.
Pure cresote, from beech wood	ml. 45.
Glycerin neutral	3 17 1/2.
Alcohol	3 2 1/2.
Distilled water	3 5.
Hydrochlorate of morphia	gr. 1 1/2.

Dose.—Fifteen minims, repeated several times (as many as four or five times) daily, deeply injected.

Tar Water in Hemorrhage.—DR. CORNEILLE DE SAINT MARC finds that distilled tar water has a haemostatic effect very similar to that of hamamelis. When prepared with the tar of pine wood it has valuable tonic astringent properties. It may be administered in quantities of from 10 to 15 drachms, during the twenty-four hours, in congestive pulmonary hemorrhage, and in hemorrhage of the uterus and kidney. It arrests the hemorrhage of the first two stages of phthisis with remarkable promptitude.—*Lancet*, May 12, 1888.

THE MEDICAL NEWS.

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OF MEDICAL SCIENCE.

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SATURDAY, JUNE 23, 1888.

THE CASE OF THE LATE EMPEROR OF GERMANY.

SINCE our remarks, in our issue of December 10, 1887, the telegraphic press reports of the medical aspects of this historical case have been so unsatisfactory, in a scientific point of view, that we have purposely refrained from further comment. The lack of information as to the laryngoscopic features of the disease since the performance of the tracheotomy has prevented any accurate interpretation of the meagre and conflicting press dispatches as to the progress of the malady. Now that the case has terminated, and the post-mortem examination has shown the disease to have been carcinoma, some very interesting problems are near their solution.

In our last remarks referred to, the observation was made that "if the disease in the larynx of the Crown Prince of Prussia be carcinoma, the clinical course has been different from what is usual in that disease." It would be injustice to the distinguished gentlemen who have had professional charge of this case to enter into any criticism of their opinions, or of their reports, until the entire clinical history shall have been written by them. In view of a statement that the physicians in attendance upon the Emperor, and notably Sir Morell Mackenzie, denied that the disease was cancer, because of certain clauses in the Prussian Constitution debarring princes afflicted with incurable maladies from accession to the throne, we have only to refer to Sir Morell

Mackenzie's candor in his official report made at the request of his patient, and published in *The British Medical Journal* of November 19, 1887; in concluding which he says that "although the nature of the growth which has lately appeared has not been determined by microscopic examination, it presents every appearance of cancer." Surely nothing could be more explicit. If he really became more guarded in his later expressions, it was certainly for good reasons that he felt himself precluded from expressing his views more frankly; and, in justification of such a course for State reasons, he had doubtless the highest advice and authority.

Two important questions of scientific interest present themselves. The one is: Was the disease malignant from the outset? The other is: Supposing that the disease was malignant at the period at which Mackenzie's advice was sought, thirteen months before death, would it have been better practice to excise the larynx?

The fragments of growth removed intralaryngeally from time to time by Sir Morell Mackenzie revealed no malignant features under the microscopic scrutiny of Virchow—the acknowledged master of pathology. In commenting upon his official reports as to the benign character of these fragments, and while distinctly mentioning that that character was no accurate criterion as to the nature of diseased structures still *in situ*, Prof. Virchow made the statement (*Berliner klin. Woch.*, December 21, 1887) that in an examination, expressly instituted for the purpose, of such specimens of cancer of the larynx as he had been able to gather from several collections, he had failed to find a single instance of independent warty formation in association with cancer. This stamps the case under consideration as unique in that respect.

It is not at all probable that any topical interference with the disease, whether electric cauterization practised by Gerhardt, or the subsequent evulsions of morbid growths by Mackenzie, had any influence whatever in transforming a presumptively benign neoplasm into a malignant one. Occasional instances of such transformation are on record, but the correctness of such views is a matter of great doubt. Hence the strong probability that the disease was malignant from the outset.

Whether laryngectomy would have removed the disease so thoroughly as to prevent recurrence of carcinoma, and thus have prolonged life for a considerable period, is highly doubtful in the light of the post-mortem investigation. The press report

of this examination is of no scientific value, but it is sufficient to show that the disease was very extensive. Official reports must be awaited to learn the character of the changes which had taken place, and the extent to which the perilyngeal and peritracheal structures had become involved; but the press report is sufficient to indicate that laryngectomy would have failed to prolong life for as long a period as has followed the palliative and symptomatic treatment which was adopted. Unfortunately, tracheotomy prolonged life but for a very few months; while ordinarily it prolongs life from several months to many. This result is a further indication that laryngectomy would have failed in this instance to be of material benefit.

One comment that suggests itself is whether the amount of official work and worry which seems to have been crowded upon the late Emperor may not have had a good deal to do in so sapping his recuperative powers as to prevent him from reaping that prolongation of life which might have reasonably been expected in a man of his physical vigor at the time of the performance of the tracheotomy. He seems to have been engrossed in his official duties to the very last moment of tolerance, almost to the very day of death.

On the whole, we must come to the conclusion that the case of the Emperor has been handled with consummate ability, and that every untoward feature has been promptly met by Sir Morell Mackenzie in the most judicious manner, and that professional skill did all that human power could do to prolong to the utmost limit the lamented Emperor Frederick's waning life. The pluck and endurance of the patient, and his unflinching confidence in his chief medical attendant, have been such as to command the admiration of the whole world.

ABDOMINAL TUMORS IN THE NEGRO RACE.

IT is well known to those who have had to deal with the negro race as it exists in our Southern States, that there is a difference in the susceptibility of negroes to the development of tumors from that of the whites; and this is especially true in regard to neoplasms of the female generative apparatus. Myomatous tumors of the uterus are exceedingly common in colored women; so much so, that it is rather uncommon to find a womb in a middle-aged negro cadaver, in which myomata have not developed. On the contrary, cystic tumors of the uterus or ovaries are very uncommon in the negro; but this

infrequency must not blind us to the fact that such growths do occur, and probably more frequently than is supposed. The infrequency of cystic growths in this race has hitherto been considered a sufficiently diagnostic feature to exclude these tumors in a doubtful case, but we can no longer rely upon the statement of authorities that such growths are "almost unknown."

The above remarks are suggested by the fact that within the past few days we have seen an undoubted case of large multilocular ovarian cystoma in a negro woman, the character of the tumor having been verified by its removal by laparotomy.

In the *Maryland Medical Journal* of June 2, 1888, DR. WILLIAM PAWSON CHUNN, of Baltimore, has a suggestive article upon "Abdominal Tumors in the Negro Race," in which he shows conclusively by his own experience and that of Dr. Hunter McGuire and others, that cystomatous disease of the uterus and ovaries is far from being unknown. Renewed attention is called to the fact that cystomata do occur, and that we need not hesitate to make the diagnosis of ovarian tumor, if the symptoms correspond with those of that disease.

DR. DAVID W. CHEEVER has been elected President of the Massachusetts Medical Society for the ensuing year.

At the annual meeting of the New Jersey State Medical Society, held last week, Dr. H. Genet Taylor, of Camden, was elected President for the ensuing year. The next meeting will be held at Spring Lake, in June, 1889.

ACCORDING to the *Maryland Medical Journal* of June 16, 1888, thirty-four graduates of the College of Physicians and Surgeons of Baltimore have appeared before the Virginia Medical Examining Board since January 1, 1885, and of these ten failed to pass the examination.

THE University of Bologna, on the occasion of the eight hundredth anniversary of its foundation, conferred its honorary degree upon several representatives of literature, science and art, and included in the list was Dr. S. Weir Mitchell, of Philadelphia, who represented the National Academy of Science on the occasion.

THE valuable addition to the structures at Princeton College of a new biological laboratory is an

accomplished fact. The building has been completed and is now being fitted out and made ready for occupancy at the coming fall session. The formal presentation of the property to the board of trustees will take place during the Commencement week. The donors of this important contribution to the scientific instruction of the college are the alumni of the class of 1877, and by their name the building will be known hereafter. Of the members of that class, three, at least, Professors Libbey, Scott and Osborn, have been identified with the upgrowth of the sciences on conservative Jersey soil.

PROFESSOR WILLIAM H. WELCH, M.D., of Johns Hopkins University, will deliver before the Yale Medical Society, New Haven, an "Address in Medicine," on June 26th.

The Canada Medical and Surgical Journal announces that on the first of July it will assume the name of *The Montreal Medical Journal*, will increase the size of its monthly issue by sixteen pages, and reduce its subscription price to two dollars. *The Canada Journal* has always been among the most welcome of our exchanges, and in its new departure we trust that it will receive the large measure of success which its enterprise deserves.

DR. A. C. HENDERSON, of Brooklyn, recently lost a son by a strange and sudden form of death. The lad, aged about twelve years, was assisting in the removal of a large iron retort containing strong commercial ammonia, such as is used in the artificial production of ice. The wagon, upon which the retort was transported, had been jolted much and incautiously and the receptacle was burst, the contained liquid being thrown over the face and body of the lad. He was burned most severely and suffered terribly, death ensuing in about twelve hours. It is altogether probable that if a greater degree of caution had been exercised in the transportation of the dangerous liquid this promising young life would not have been lost.

AN intercolonial commission on the rabbit pest of Australia has recently been appointed, composed largely of medical men: Dr. H. N. MacLaurin, President of the Board of Health of New South Wales, for that colony, with Dr. C. Wilkinson; Professor Allen for Victoria; Drs. Stilling and Patterson for South Australia; Dr. J. Bancroft for Queens-

land. A representative of New Zealand will be admitted to the commission. Dr. Camac Wilkinson, a member, is the author of a proposition to try Pasteur's plan for the destruction of the pest by inoculation, upon an isolated, uninhabited island which shall be stocked with rabbits and every form of useful animal found in the colonies, and test the method in a precise and thorough manner before adopting it on a wider scale.

A MONUMENT to the memory of Cohnheim was unveiled at Leipzig, with appropriate ceremonies, on the 3d of June.

AT the Second Congress of the German Society for Gynecology, held at Halle on the 24th of May, it was determined, in view of the meeting of the International Medical Congress in 1890 at Berlin, that the Third Gynecological Congress meet next year at Freiburg.

PROF. R. V. JAKSCH has been elected Chief Physician to the Anna Children's Hospital at Gratz.

THE Managers of the Pennsylvania Hospital last Saturday afternoon, formally opened a new villa for the treatment of the insane, which has just been erected, under the supervision of Dr. Chapin, on the grounds of the institution in West Philadelphia. The cottage is of an attractive style of architecture, and its interior arrangements and furnishing are both home-like and handsome. It will accommodate about sixteen patients. The erection of this building will enable those in charge of the institution to form an intelligent judgment as to the value of the cottage-plan in the treatment of the insane, and the result will probably be seen in the character of the buildings which, when the time comes, will be erected on the extensive, new grounds recently purchased in Delaware County.

ON October 26th, the International Society of the Red Cross will celebrate the twenty-fifth anniversary of its organization.

THE Canadian Medical Association will hold its twenty-first annual meeting in the city of Ottawa, on 13th and 14th of September, under the presidency of Dr. J. E. Graham, of Toronto.

AN International Congress for Dermatology and Syphilography will be held in August, 1889, at Paris. Its sessions will continue one week, and will be presided over by Ricord and Hardy.

SOCIETY PROCEEDINGS.

PHILADELPHIA COUNTY MEDICAL SOCIETY.

Stated Meeting, May 23, 1888.

THE PRESIDENT, J. SOLIS-COHEN, M.D., IN THE CHAIR.

DR. A. JACOBI read a paper on

THE THERAPEUTICS OF DIPHTHERIA.

(See THE MEDICAL NEWS, June 16th, page 654.)

DR. JOSEPH O'DWYER then read a paper on

THE USE OF INTUBATION TUBES.

After an experience with tubes of various sizes in over two hundred cases of croup, besides other forms of stenosis, in children, he is fully convinced that, as at present constructed, the intubation tubes afford ample room for carrying on the respiratory function in the most perfect manner. When the disease is confined to the larynx and upper portion of the trachea, it is not an uncommon experience, after the paroxysm of coughing that immediately succeeds intubation has subsided, to find the little patient breathing so quietly and imperceptibly that it is sometimes difficult to convince the mother, who has returned to the room after an absence of fifteen or twenty minutes, that her child is still living. Such complete freedom of respiration would be impossible were the opening too small. When the struggle for breath has continued long enough to produce extreme exhaustion, together with more or less atelectasis and congestion of the lungs, this perfect relief does not occur. The same is true after the partial asphyxia induced by prolonged or repeated attempts to insert the tube. Such cases sometimes never rally, although air enter the lungs in the freest possible manner. If any dyspnea whatever remain for any considerable time after intubation, or if the respiration be much above the normal in frequency, it indicates the presence of some complication or extension of the disease below the tube. The fact that several times on removing a tube from the larynx he has found its calibre considerably reduced by firmly adherent secretions, when there had been no dyspnea to indicate it, is good evidence that there is more room than is actually required for the free entrance and exit of air.

He then referred to the manner in which the machinery concerned in the removal of secretions is modified or injured by a canula in the larynx or trachea. The mechanism of coughing, as he understands it, is simply getting as much air into the lungs as possible, condensing it, and allowing it to escape suddenly, on the same principle as the air-gun. To accomplish this, the glottis is firmly closed, coincidently with spasmodic contraction of the expiratory muscles, until the imprisoned air is sufficiently compressed, not only to give it power to project any offending substance before it, like the ball from an air-gun, but also to increase the friction between it and the lining membrane of the air-passages to such a degree as to scrape off, so to speak, secretions that may be adherent.

Considerable condensation, with great velocity of the expired air, is, therefore, necessary to give the maximum expulsive power. The latter without the former would accomplish nothing, because the same volume of air can

be driven through the open or half-open glottis just as rapidly as in the act of coughing, without the least power to remove a particle of mucus even from the larynx, much less from the bronchial tubes. This can be demonstrated by trying to cough while retaining the vocal cords in the expiratory position—the lack of power resulting solely from inability to compress the air to any appreciable extent.

Coughing through a canula is identical with this act when performed with a partially open glottis, and the only means left of subjecting the air to any condensation whatever is the much shorter time occupied in expelling it through the same space by which it more slowly entered.

An excellent and forcible illustration of this argument, and one the mechanism of which is identical with that of coughing, is the familiar act of blowing the nose. There is little or no ability to remove secretion from this organ without first reducing the nostrils to a small fraction of their normal calibre, or by momentarily producing complete occlusion, as in closing the glottis, until the air is sufficiently condensed to force the secretions out with it. Very little power can be developed even by closing one nostril and forcing all the air through the other, if normally patent. If secretions can be removed more effectually from the air-passages through a canula of the dimensions advocated by the authorities already quoted, for the same reason it should be easier to remove accumulations from the nose without compressing the nostrils.

He claimed, therefore, that while the artificial opening must be large enough for the perfect performance of the respiratory function, the power to expectorate is still further diminished, and in exact proportion to its increase beyond this limit.

DR. WILLIAM PEPPER said that the extreme value of iodoform in local treatment he could confirm. Soluble in ether, miscible with glycerin and with oils, capable of use as powder, it is the best of all local applications, and may be applied to all cases and conditions. He would, however, interpose a mild protest against the too sweeping condemnation of the steam atomizer. Used with that gentleness, so wisely insisted upon, and the confidence of the child obtained, the relief to oppression is soon recognized, so that we can secure intelligent co-operation in its frequent and regular use.

The problem of internal treatment is the most difficult one, a problem which largely and continually occupies our thoughts. He was glad to hear Dr. Jacobi's clear and outspoken adherence to the mercurial treatment, although he limits it to a certain group of cases. His longer experience has but abundantly confirmed his early impressions of its value—preëminently in laryngeal diphtheria, whether primary or descending; and he is constantly impressed with the tolerance of children to the bichloride, and equally to the mild chloride. But he would go further. If in a form conspicuous as a dangerous one, which is usually not primary but associated with rhinitis and fauces, this treatment proves efficacious, why is it not equally good where the nasal or faecal disease has not extended into the larynx? He has found cases of nasal diphtheria which were a source of great anxiety yield in a most remarkable way, and it has seemed to prevent the local spread as well as septic infection.

He agreed with Dr. Jacobi that it is well to begin treatment with the chloride of iron, and that the association of chlorate of potassium is a matter of comparative indifference, and that large doses should be given at short intervals. But he had not been so fortunate in seeing it usually well borne by the stomach. When gastric or intestinal irritation manifests itself, it is well to stop the iron abruptly and to substitute mercurials. Or, when in the beginning of a case of glandular involvement, the faecal tumefaction, the constitutional symptoms, give evidence of rapid sepsis, we cannot depend upon iron and must give the corrosive or the mild mercurial chloride at once.

DR. CARL SEILER said that in his experience the addition of chlorate of potassium to the chloride of iron has been of great use, although he agreed with Dr. Jacobi that chlorate of potash alone is of little use. From laboratory experiments he attributed this to the disengagement of chlorine gas when the two solutions are mixed. In the same way, at the suggestion of Dr. L. Wolff, he finds Labarraque's solution an efficient disinfectant in the proportion of 1 to 5. He uses this as a spray to the nasal cavities or fauces, and has had excellent results.

DR. E. E. MONTGOMERY said that since August, 1886, he had performed some thirty or forty intubations, having previously done some twenty-eight tracheotomies. Fifty per cent. of the children intubated have recovered. His experience is that this operation largely reduces the necessity for tracheotomy, and he believes that if intubation were done early in every case, tracheotomy would rarely be necessary.

DR. M. PRICE said that in the evolution of steam from lime he had for the last fifteen years depended upon the same method as country people use in the scalding of hogs. Put a few pieces of lime in a bucket with hot water, place a blanket over the bed and let steam pass over the child's head. Soon the child acquires confidence and asks for relief, and will even bend his head down over the bucket trying to get the vapor into his throat. Now, if every half hour a hot stone or brick or piece of metal be added to the water, it will keep up the heat without any stove or fire being needed in the room. It keeps the room clean and the atmosphere sweet; he had not found so much danger of contagion when lime is used.

DR. J. SOLIS-COHEN said in regard to local treatment that where it can be properly applied to the extreme margins of the pseudo-membrane, he had found the topical use of chloride of iron, by firm and gentle pressure with brush, or, preferably, cotton wad, the most serviceable agent he had used. The drug has an astringent and a disinfectant action, and assists the detachment of false membrane, and apparently prevents the extension of the infection. Concerning the value of chloride of iron internally, the importance of large and frequent doses, the advantage of mixture with glycerin to assist its local effect, he can only confirm what has been said. So, too, as to the bichloride of mercury.

Empirical observation has long taught us the preëminent value of the chlorine compounds in general in the treatment of diphtheria; and the mercury chlorides, more particularly calomel, however, have always enjoyed a high reputation in the internal treatment of membranous laryngitis. The topical action of steam is very important. It has always seemed to him that in the natural

course of the disease, the membrane is thrown off by an accumulation of fluid beneath it, which softens it and secures its detachment. We aim, then, by furnishing artificial moisture, to imitate the natural process of recovery. And this led him to speak of the value of the vapors from lime in the process of slaking. Using a large wash-tub or wash-boiler, and keeping up a supply of large pieces of lime, we secure an abundant disengagement of the hot vapor of water, carrying up with it particles of lime, which mechanically assist by prying up the edges of the pseudo-membrane, and thus favoring the access of the vapor of water beneath it.

There is another method of local treatment which he had employed with great satisfaction, more especially in former years, when he saw more of the disease—that is, inhalation of carbolic acid in the spray of a steam atomizer, in very large doses. Twenty to twenty-five grains would be added to the ounce of water, and from half an ounce to an ounce sprayed into the throat every hour, or even half hour, until commencing discoloration of the urine gave evidence of saturation, when the remedy was to be stopped until the urine again became clear. Under this method he would advise the attending physician to see the child four or five times a day, always having the urine last voided saved for him, and when the olive discoloration was noticed to intermit the carbolic acid. This seemed to him to disinfect the system, and thereby improve the local condition, and, at the same time, to prevent or diminish the danger of systemic sepsis. He was not aware, before to-night, that such small doses of carbolic acid, as Dr. Jacobi mentioned, could be of service.

His own experience with tracheotomy led him to favor large tubes, the largest that can be introduced without touching the walls of the trachea. He still believes that he has seen life saved by taking out small tubes and substituting larger ones. But facts are stronger than theories, and as the small calibre intubation tube does seem to give air enough, and as enough is all that is wanted, he is ready to profess his satisfaction with its present calibre.

He asked Dr. O'Dwyer to make clear the question as to the impaction of membrane. Some years ago he was called to a case of membranous laryngitis, and had proposed tracheotomy, which had been declined. As he turned to leave the room the mother called piteously, "Oh, Doctor, don't leave my child without trying to do something for it." He said to his assistant, "we will try to save this child," and taking a catheter he cut off the end, and passed the instrument into the larynx. The child instantly became black in the face, and there was nothing for it but, without asking any questions, to plunge his knife into the trachea as the child lay on its mother's lap. He inserted the same catheter through the orifice deep into the trachea, and then he performed artificial respiration; his assistant inflating the child's lungs through the tube with his own breath, and his hands exercising compression of the thorax in respiratory rhythm; and, after a while, he had the satisfaction of leaving the rescued child sleeping peacefully with unobstructed respiration. But this experience cost him some of the most anxious moments of his life, and has left a fear of the danger of crowding down membrane in front of a tube introduced into the larynx, which may, perhaps, make him overanxious.

DR. JACOBI said that the slaking of lime has the further advantage that it is the only way to utilize lime. A lime-water spray is useless, but in slaking a large amount is carried up into the air and air-passages. The suggestion of the President that carbolic acid should be used in spray until discoloration of the urine is noticed, he did not feel inclined to adopt. Diphtheria is the very disease in which no complications should be allowed to exist, and we must not tempt them. A single case in which we should have to blame ourselves for a possible nephritis would, in his judgment, condemn the treatment. Besides, young infants are sometimes poisoned by very small quantities.

DR. O'DWYER said that pushing down of membrane does occur, though rarely. The difference between the liability to the accident in catheterization and intubation, is that the catheter has an open, comparatively broad end, while the intubation tubes are comparatively probe-pointed. One pushes and catches the membrane, the other slides past it. He has crowded membrane down in only two cases out of two hundred sufficiently to produce asphyxia. In those two, on removal of the tube, the cast was coughed out. If we take away the tube because the child is breathing badly, and the trachea is full of membrane, the child, not having the strength to cough it out, chokes from the absence of the tube, not from its previous presence. His attention is now being directed to devising a means to get rid of the membrane, and he hopes to present something practical before long. Blocking with membrane, while the tube is in, may occur. Formerly, when the swell of the tube was not so great, it would be coughed out, but now it is not coughed out, and suffocation may take place. The original tube was better in this regard.

The earlier tubes were made to fit into the ventricles with the idea of permitting the approximation of the ventricular bands, but it did not work. It is true that the epiglottis is merely an accessory, but in an intubation case the ventricular bands being held open we have to depend upon it; and that is the reason, the dependence being a poor one, that solids and semisolids which can go down in mass are better than liquids.

NEW YORK NEUROLOGICAL SOCIETY.

Stated Meeting, June 5, 1888.

THE PRESIDENT, GEORGE W. JACOBY, M.D.,
IN THE CHAIR.

DR. FREDERICK PETERSEN read some

NOTES ON THE PRINCIPLES OF CRANIOMETRY.

After a review of craniometric nomenclature the reader stated that, while separate convolutions exert no specific influence upon the bones of the head, the shape of the skull is modified in correspondence with the gross divisions of the brain beneath it. The left temporal bone was said to be depressed in congenital aphasia. In infantile spastic hemiplegia there was flattening of the side of the skull opposite to the paralyzed part. Cerebral localization has been concerned mainly with motor and sensory functions. Ideational localization had yet to be developed. In his own opinion the temporo-sphenoidal lobes, and perhaps the occipital, contained cortical centres for depressing emotions. Musical ideas

had their origin in the temporo-sphenoidal lobes together with the rest of the auditory memories.

Benedikt has reduced craniometry to a science, showing that the skull is built upon crystallographic principles. The measurements taken should be sufficient to reconstruct the skull. Triangulation of the skull should be required in asylums in the case of every patient, and in prisons in the case of every criminal. We are behind European countries in this matter. Even in Italy fourteen measurements are required for asylum records. The reader thought eleven measurements at least should be made: 1, the circumference of the skull; 2, the naso-occipital arc; 3, the naso-bregmatic arc; 4, the bregmatic lambdoid; 5, the binauricular; 6, the antero-posterior diameter, taken from the glabella to the maximal occipital point; 7, the greatest transverse diameter; 9, the two auriculo-bregmatic radii; 10, the facial length; 11, the greatest height of the skull. A calipers, tape-line and a strip of lead two feet long only are required. For more detailed measurements other instruments are required. Benedikt's calipers were recommended.

The pathological and forensic importance of such measurements are shown by the fact that minimal and maximal measurements are more common among the insane and criminal classes than among other people. The bregmatic-lambdoid arc was said to be shortened in epilepsy. The reader referred to one hundred cases of his own observed at the Hudson River State Hospital, at Poughkeepsie, in which asymmetry had been observed.

The measurements do not present any practical difficulties; the eleven measurements can be obtained and a drawing made in the course of fifteen or twenty minutes. In measuring the length of the parietal bone care must be exercised in getting the points correctly, but only a tape measure is needed.

DR. J. A. BOOTH presented the report of a case of PROGRESSIVE MUSCULAR ATROPHY WITH ANÆSTHESIA.

The patient was forty-two years of age, a shoemaker by trade, and still under observation. There was no family history of nervous disorders, no history of alcoholism or syphilis in the case. The patient had been married eighteen years, and had had two children; one, a girl of five years, had never walked. The affection had commenced in October, 1878, with general weakness, debility and weakness in the arms and hands. In January, 1879, the patient's voice had commenced to be husky. Six months later he had developed a feeling of cold and numbness in the left shoulder and side of the neck, with subsequent decrease in size. The atrophy commencing in the deltoid had spread to other muscles of the trunk and upper extremities. At the present time the patient weighed 155 pounds. There was marked sinking of both shoulders. There was weakness of the upper extremities with marked atrophy of the interossei. There were scars and abrasions about the hands and a scar upon the neck. The patient stated that he did not know when these injuries were received, that he had not felt them. There was no ataxia in walking or upon standing. The voice was harsh, the left side of the palate was paretic, the uvula being drawn to the right. The larynx had been examined by Dr. A. H. Smith. The left superior constrictor, the left palato-pharyngeus and the adductors of the left vocal cord were paralyzed. There was slight deviation of the tongue to the right.

There were marked fibrillary contractions in the atrophied muscles. The patellar reflexes were exaggerated. There was sluggishness of the accommodation, but no change in the visual field and no diplopia. Taste, smell and hearing were not impaired. It was apparently a case of progressive muscular atrophy with bulbar symptoms. The reader called attention to the sensory impairment as an unusual complication, and suggested to account for the anaesthesia and analgesia a lesion in the peduncle or pons on the right side.

DR. STARR found the case interesting, but remarked that the anomaly mentioned had been recorded by Ross in his text-book, and by Gowers in his text-book. In cases of this character post-mortem examinations had shown abnormal cavities in the cord, due chiefly to the degeneration of gliomatous tumors. Schultze had described cases; Bäumler also in his article upon syringomyelia. The case reported by Dr. Booth was, in his opinion, a case of this kind. The fact that the senses of touch, pain and temperature were all abolished would support this view. Touch sometimes escapes in syringomyelia, but not always. The three tracts are found in the formatio reticularis of the medulla and pons, and extend through at least one-fourth its extent. A lesion affecting them all would involve also the cranial nerves passing through this part. The symptoms reported could be more satisfactorily explained by a lesion in the cord, and the case as one of syringomyelia.

DR. SACHS had the same impression of the case, the combination of sensory and motor symptoms being a prominent feature in syringomyelia. From the fact that the anaesthesia had not affected the leg, it appeared that the grouping was exactly similar to that in this disease.

DR. PETERSEN asked whether the designation syringomyelia could be proper where cranial nerves were involved.

DR. SACHS replied that the cavity might extend upward into the medulla; that a number of cases of syringomyelia had been reported in which the cavity had involved the medulla. In regard to the question of nomenclature, we were not yet in a position to say whether the cord ended below the medulla or above it.

DR. BOOTH asked whether in these cases there was paralysis of the limbs.

DR. STARR replied that there was paresis proportioned to the atrophy, not paralysis in the sense of hemiplegia or monoplegia.

DR. J. C. SHAW followed with a paper upon

DEGENERATION OF THE PERIPHERAL NERVES IN LOCOMOTOR ATAXIA, WITH MICROSCOPIC SECTIONS.

The patient, forty-seven years of age, had been a typical case of locomotor ataxia. Following an attack of hematuria he had had two epileptic seizures, and had died the following day. On post-mortem examination the hemorrhage was found to have come from the right kidney. Pieces of the sciatic, plantar and popliteal nerves had been removed for examination; part of the specimens being stained with osmic acid, and part with the bichromate of potash solution. Changes were found which the reader considered distinct from the Wallerian degeneration. The changes consisted in granular degeneration, liquefaction and even absorption of the myelin sheath, with persistence of the axis-cylinder, and in some places a collapsed sheath of Schwann.

NEWS ITEMS.

American Association of Obstetricians and Gynecologists.—The following preliminary programme of the Annual Meeting, to be held in Washington, D. C., September 18, 19 and 20, 1888, has been announced:

The President's Annual Address. By William H. Taylor, M.D., of Cincinnati.

Discussion: Extrauterine Pregnancy. 1. Pathology.

2. Diagnosis. 3. Treatment. By Mr. Lawson Tait, Dr. Franklin Townsend, Dr. E. E. Montgomery, Dr. Charles A. L. Reed, Dr. A. Van der Veer.

The Relations of the Abdominal Surgeon to the Obstetrician and Gynecologist. By Albert Van der Veer, M.D., of Albany.

Operation for an Unusual Case of Subserous Uterine Fibroid. By Hampton Eugene Hill, M.D., of Saco, Me.

Drainage in Abdominal and Pelvic Surgery. By Joseph Price, M.D., of Philadelphia.

Double Ovariectomy during Pregnancy; a Successful Case Going on to Full Term. By William Warren Potter, M.D., of Buffalo.

The Indications for Artificial Aid in Labor. By Thomas Opie, M.D., of Baltimore.

The Technique of Vaginal Hysterectomy. By James H. Etheridge, M.D., of Chicago.

The Surgical Treatment of the Perineum. By William H. Wathen, M.D., of Louisville.

Laparotomy in Peritonitis. By E. E. Montgomery, M.D., of Philadelphia.

Tumors of the Abdominal Wall. By Charles A. L. Reed, M.D., of Cincinnati.

Uterine Fibroids; their Diagnosis and Treatment. By Thomas J. Maxwell, M.D., of Keokuk.

Dermoid (Fibroid) Tumors of the Abdominal Walls. By Edward J. Ill, M.D., of Newark.

Ruptured Perineum. By J. Henry Carstens, M.D., of Detroit.

A Contribution to the Study of Pelvic Abscess. By Clinton Cushing, M.D., of San Francisco.

The Female Perineum: its Anatomy, Physiological Function and Methods of Restoration after Injury. This paper will be illustrated with lime-light and screen. By Henry O. Marcy, M.D., of Boston.

Heart Failure in the Puerperium. By Thomas Lothrop, M.D., of Buffalo.

Treatment of Suppurative Peritonitis. By William H. Myers, M.D., of Fort Wayne.

Operative Treatment in Uterine Carcinoma. By George R. Shepard, M.D., of Hartford.

The Reflexes Reflexed; or Some Things that Retard Progress in Gynecic Surgery. By Joseph Eastman, M.D., of Indianapolis.

Some Points in Relation to the Diagnosis of Pregnancy in the Early Months. By James P. Boyd, M.D., of Albany.

Vaginal Tamponnement in the Treatment of Prolapsed Ovaries. By W. F. Manton, M.D., of Detroit.

Mr. Lawson Tait, F.R.C.S.E., of Birmingham, England, will also present a paper on "The Methods of Success in Abdominal Surgery."

A Testimonial to Mr. Bryant.—On May 31st, in the Anatomical Theatre of Guy's Hospital, on the occasion of

his retirement from the surgeonship, a testimonial, from past and present students, in the form of a handsome silver punch-bowl, with an illuminated book, containing photographs of various parts of the hospital and the names of the subscribers, was presented to Mr. Bryant.

Test for Plumbing.—The New York Board of Health has led the way for other municipal authorities by adopting a mandatory test for plumbing. On and after July 1 the work under plans filed after that date must be tested by the plumber in the presence of the inspector, by means of an air pump and pressure-gauge, in the same manner as the gas-pipe system of the building. This is claimed to be the most effective test possible, as the pressure is equal all over the building. The department of public works will issue no permit for water for domestic uses until the certificate of the health department is presented.

A Milk Epidemic.—An important investigation under the direction of Dr. Russell, health officer of Glasgow, Scotland, is in progress. It is no less a matter than the alleged direct infection of a number of people in the West End of Glasgow with scarlet fever, by means of milk from a dairy farm. The cattle on the farm in question were found "slightly ailing" with the very disease which, when transmitted through milk or meat to human beings, brings on scarlet fever. The result was a severe epidemic. No other epidemic, of late years, has offered such a field for investigation, and Dr. Russell's work will be followed with a great deal of interest.

Still Another Test for Sugar.—Fischer detects glucose in the urine by the formation of crystals of phenyl-glucosazine. A little phenyl-hydrazine hydrochlorate is put into a test-tube, together with twice its weight of sodium acetate; water is then added and heat is applied. An equal volume of the urine to be examined is now added, and the mixture is boiled twenty minutes and then cooled, when yellow crystalline needles separate if glucose is present. When the quantity of glucose is minute, it is necessary to have recourse to the microscope to find the characteristic crystals. By the aid of this reagent Van Jakob has been able to show that sugar is present in the urine after inhalation of chloroform, in cases of asphyxia, after taking large doses of sodium salicylate, etc., where the reaction with Fehling's solution is in the nature of things inconclusive.—*Pharmaceutical Era*, June, 1888.

The Epidemiological Society of London, at its meeting of May 24th, elected Max v. Pettenkofer an honorary member.

A Risk of Travel.—The reported indisposition of the Duke of Edinburgh, from drinking impure water at a foreign station, gives prominence to what is perhaps the most usual and frequent source of danger in foreign and Continental travel. Many of the sanitary authorities who have looked into the question have, from time to time, uttered warnings to Continental travellers as to the dangers of the ordinary drinking water to be found abroad. The pollution of table water at foreign hotels and houses is due to a great variety of causes. The water supply of

foreign cities is, as a rule, to which there are only few exceptions, taken from sources lamentably liable to sewage pollution, either in open streams or uncovered reservoirs, or from defective sanitation in the house supply. A large part of the domestic supply of drinking water is, moreover, from surface wells, which are constantly liable to sewage filtration. An examination, made only a few years since, of siphons of sparkling "seltzer" in a great Continental city, disclosed the fact that they were horribly polluted with sewage, and that the effervescing fixed air with which they were charged only served to conceal unutterable contaminations of a most dangerous kind. Sir Henry Thompson and Dr. Herman Weber, who have both given attention to the subject, are very emphatic in their counsel to travellers to avoid ordinary drinking water abroad. The easiest and most agreeable means of avoiding the danger is the habitual use of a pure natural mineral water in lieu of the doubtful drinking water of the hotel or the private house.—*British Medical Journal*, June 2, 1888.

OFFICIAL LIST OF CHANGES IN THE STATIONS AND DUTIES OF OFFICERS SERVING IN THE MEDICAL DEPARTMENT, U. S. ARMY, FROM JUNE 12 TO JUNE 18, 1888.

WATERS, WILLIAM E., Major and Surgeon.—Granted leave of absence for twenty-days, to take effect about June 15, 1888.—*S. O. 64, Department of Columbia*, June 8, 1888.

CALVIN, DEWITT, Major and Surgeon.—Granted leave of absence for one month, with permission to apply for an extension of one month.—*S. O. 39, Department of Dakota*, June 9, 1888.

BIRMINGHAM, HENRY P., Captain and Assistant Surgeon.—Granted leave of absence for one month.—*S. O. 117, Division of the Atlantic*, June 11, 1888.

OFFICIAL LIST OF CHANGES IN THE STATIONS AND DUTIES OF THE MEDICAL CORPS OF THE U. S. NAVY, FOR THE WEEK ENDING JUNE 16, 1888.

MCMURTRIE, DANIEL, Surgeon.—Detached from the Naval Hospital, Yokohama, Japan, and ordered to return home.

GRAVATT, C. U., Surgeon.—Detached from U. S. S. "Michigan," and ordered to the Hospital at Yokohama.

EDGAR, JOHN M., Passed Assistant Surgeon.—Ordered to the U. S. S. "Michigan."

MACKIE, B. S., Surgeon.—Ordered to the U. S. S. "Ossipee."

PARKER, J. B., Surgeon.—Detached from the U. S. S. "Ossipee," and await orders.

OFFICIAL LIST OF CHANGES OF STATIONS AND DUTIES OF MEDICAL OFFICERS OF THE U. S. MARINE-HOSPITAL SERVICE, FOR THE WEEK ENDING JUNE 16, 1888.

WYMAN, WALTER, Surgeon.—Detailed as a member of the Board of Examiners, vice Surgeon Fessenden, excused on account of physical disability. June 15, 1888.

FESSENDEN, C. S. D., Surgeon.—Granted leave of absence for thirty days, on account of sickness, June 15, 1888.

THE MEDICAL NEWS will be pleased to receive early intelligence of local events of general medical interest, or of matters which it is desirable to bring to the notice of the profession.

Local papers containing reports or news items should be marked. Letters, whether written for publication or private information, must be authenticated by the names and addresses of their writers—of course not necessarily for publication.

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